Q51
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Ser. 2
v. 32
Nov.
1861

THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

PROFESSORS B. SILLIMAN, B. SILLIMAN, Jr.,

AND

JAMES D. DANA,

IN CONNECTION WITH

PROF. ASA GRAY, OF CAMBRIDGE,
PROF. LOUIS AGASSIZ, OF CAMBRIDGE,
DR. WOLCOTT GIBBS, OF NEW YORK.

SECOND SERIES.

VOL. XXXII.—NOVEMBER, 1861.

NEW HAVEN: EDITORS.
1861.

PRINTED BY E. HAYES, 426 CHAPEL ST.

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ERRATA.

P. 37, 3d line from bottom, *for* "major axis, *read* semi-major axis."
P. 109, 10th line from bottom, *for* "Honzeau," *read* "Houzeau."
P. 416, 17th " " top, *for* "nearly," *read* neatly."
P. 440, top, *for* "Notices of Meteoric Masses," *read* "Notices of Meteors and Meteoric Masses."

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. I.—*Researches of Pasteur respecting the Theory of Spontaneous Generation;* translated and condensed by M. C. White, M.D.

The theory of spontaneous generation was long since proposed to account for the origin of beings whose germs were too minute or too obscure to attract attention. One after another the different organisms supposed to arise from spontaneous generation have been proved to originate from germs. At present the question of spontaneous generation concerns only the origin of *entozoa* and those minute organisms which can be studied only with the aid of the microscope, as moulds, (minute fungi), and infusoria both animal and vegetable. The common theory that the spores or germs of these minute organisms are constantly floating in the atmosphere ready to start into activity whenever they meet with a suitable nidus has found an able advocate in M. Pasteur of the Normal School of Paris, who has published in the *Comptes Rendus*,* a series of valuable papers on this subject, the substance of which I have translated for this Journal.

In order to collect and examine the solid particles floating in the atmosphere Pasteur placed soluble gun cotton in a glass tube and by means of an aspirator caused a current of atmospheric air to pass through it for several hours. The cotton was then dissolved in a mixture of alcohol and ether, and the atmospheric dust deposited at the bottom of the fluid in a conical glass was examined with the microscope. The sediment thus collected contained grains of starch and such other dust as is ordinarily found on surfaces exposed to the air. When submitted to the action of concentrated sulphuric acid the starch was soon dissolved, while other particles remained undissolved and had all the characteristics of the spores of ordinary mucedines which are known to resist the solvent properties of concentrated sulphuric acid. [It is worthy of notice that certain minute fungi are capable of decomposing a solution of sulphuric acid. A few years since a little mould developed in the solution of sulphate of copper used for electrotyping in the department of the U. S. Coast Survey at Washington proved an intolerable nuisance. It decomposed the salt, assimilating the sulphuric acid and rejecting the copper which was deposited around its threads in a metallic form. From this it appears that sulphuric acid does not prevent but may rather assist the growth of certain fungi.—*Tr.*]

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* *Comptus Rendus*, 1860, T. L, LI.

To determine the action of atmospheric air, and of atmospheric dust upon fermentation, putrefaction and the appearance of organization, Pasteur adopted the following methods:

A flask was about half filled with a fluid consisting of water containing in solution about ten per cent of sugar and from two to seven parts in a thousand of the scum of beer. The neck of the flask was drawn out in the flame of a lamp and attached to a platinum tube $\frac{1}{25}$ of an inch in diameter which was then heated to redness. The fluid was boiled for two or three minutes to expel all air from the flask, when it was allowed to cool very gradually, and as it cooled, the air which entered the flask, was calcined and all organic germs it contained were destroyed, by passing through the red-hot platinum tube. When the flask had thus cooled to the temperature of the surrounding air the neck was hermetically sealed. The flask was then removed to an oven and kept at the temperature of 80° or 90° F. for an indefinite period without producing any organisms or undergoing any change whatever.

To test the influence of atmospheric dust upon a fluid thus hermetically sealed, Pasteur placed a pledget of cotton or asbestos in a small tube and caused a current of common air to pass through it by means of an aspirator. This small tube containing the cotton or asbestos loaded with atmospheric dust, was then transferred to a larger T-shaped tube, one end of which was connected by India rubber with the sealed flask, another end was connected with a platinum tube heated to redness, and the third being connected with an aspirator the apparatus was easily charged with calcined air and all the common air was expelled. The neck of the flask was then broken within the T-shaped tube, and the small tube containing the atmospheric dust was passed into the flask with access only of calcined air. The neck of the flask was then again hermetically sealed by means of the blowpipe. Many flasks were prepared in this way and in every case after standing in a warm situation for 24 to 36 hours, vegetation appeared in the same manner as if the contents of the flask were exposed to the open air; but the mould or mucedines appeared first in the little tube carrying the cotton which was often thus filled to its extremities. The organic growths which appeared, were the same as in flasks exposed to the open air, viz: of infusoria, *bacterium*, of mucedines the *penicilium*, *ascophora*, *aspergillus* and some others. When calcined asbestos alone was introduced no vegetation appeared.

It was thus demonstrated that among the dust suspended in ordinary air there are always *organized* corpuscles, and that these powders when mixed with a suitable liquid in an atmosphere of itself inactive, give origin to *Bacteria* and *Mucedines* such as are furnished by the same liquid in the open air.

Pasteur confirmed these results by another method. Similar quantities of the same fermentable liquid were introduced into a series of flasks in all respects alike. The necks of the flasks were all drawn out over the flame of a lamp, and bent into a variety of different forms, but the tubular neck of each flask was left with an opening one twenty-fifth of an inch or more in diameter. In some of the flasks the liquid was boiled for several minutes, but three or four were not heated to the boiling point. All the flasks were then set away in a quiet place free from currents of air. After 24 or 48 hours, according to the temperature, the flasks in which the liquid was not boiled after being put into them, (although all the liquid had been boiled before it was put into the flasks), were found to be troubled and covered little by little with mucor. The liquid which had been boiled in the flasks remained limpid not only for days, but even for entire months, although all the flasks were left open. There can be no doubt that the curves and sinuous forms of the necks, served to secure the contained fluid from the fall of germs.

The common air entered these flasks as they were cooling, but so slowly during the gradual cooling of the hot liquid, that the germs were either destroyed by the heat, or were deposited in the curvatures of the narrow necks of the flasks so that no viable germs reached the liquid. When the neck of one of these flasks was broken off, and the remaining portion placed vertical, in a day or two the liquid became mouldy or filled with bacteria. This method which so well explains the preceding, and which can be so readily practiced by any one, carries conviction to unprejudical minds. It gives also peculiar interest to the proof which it presents to us that *there is nothing in the air except its dust which is a condition of organization.* It thus appears that oxygen acts only to sustain life furnished by germs, while of gas, fluids, electricity, magnetism, ozone, things known or unknown, *there is nothing in the air except the germs which it carries which can originate organic life.*

Fermentation of Urine.—A flask with an attenuated neck was one-third filled with fresh urine and boiled for three or four minutes and then allowed to cool with no access of air except what was drawn through a platinum tube heated to redness. When cool the flask was hermetically sealed and the enclosed urine was thus exposed only to atmospheric air deprived by heat of all viable germs. In this condition the urine remained for months without change. Into a flask thus prepared, asbestos charged with atmospheric dust was introduced by the method above described. The flask was kept at 86° F., and in about six hours mucedines and infusoria appeared, among which were *Bacteria*, *Vibriones* and *Monads*, the same as appeared in similar urine exposed to the open air. During the following days lithates and crystals of triple phosphate were deposited, the urine became ammoniacal and its urea disappeared under the influence of the true ferment of the urine, which Pasteur believes to be organized, and whose germ, could only have been introduced in the atmospheric dust in connection with the germs of infusoria and mucedines. When a flask prepared in the same manner had only calcined asbestos introduced, without atmospheric dust, neither mucedines nor infusoria appeared, neither did any fermentation take place however long the flask was permitted to remain unopened.

Coagulation of milk.—Fresh milk was boiled in a flask for two or three minutes only, and after being allowed to cool with access of calcined air, as in the preceding experiments, it was hermetically sealed. In eight or ten days the milk was coagulated, but when opened it was found remarkably different from milk coagulated in the open air for it remained *alkaline as fresh milk*; but the milk was filled with infusoria, most frequently *vibrios* about $\frac{1}{500}$ of an inch in length, yet no vegetable productions were detected.

The common theory that milk coagulates in consequence of the formation of lactic acid is an error. It is also shown that vibrios may appear in milk which has undergone ebullition for several minutes at 212° F., although urine or a solution of sugar and albumen does not produce vibrios under such conditions. In other experiments the milk was boiled for longer periods under a pressure of 1½ atmospheres at a temperature of 230° or 235° F., and the flasks were sealed as before. Flasks thus prepared furnished no infusoria, the milk did not coagulate however long it remained enclosed in the flasks, it remained alkaline even with the presence of oxygen in the form of calcined air as stated above, and it preserved apparently all the properties of fresh milk.

Into flasks of milk thus prepared Pasteur introduced atmospheric dust by the method detailed above, when the milk coagulated and both animal and vegetable productions appeared as in the milk exposed to the open air. The generally admitted theory of ferments which had of late years received fresh support from the writings of chemists, now appears more and more at variance with the results of experiments. The ferment is not a dead substance without determinate specific properties. It is a being whose germ is derived from the air. It is not an albuminous substance altered by oxygen. The presence of albuminous matters is an indispensable condition of all fermentation because the "*ferment*" depends upon them for its life. They are indispensable in the light of an aliment to the ferment. The contact of the atmospheric air is, primarily, equally an indispensable condition of fermentation, but it is indispensable only as being a vehicle for the "*germs*" of the "*ferments.*"

There are many distinct organized ferments which excite chemical transformations, varying according to the nature and organization of the ferment.

To confute various objections made by advocates of spontaneous generation, Pasteur undertook to determine the relative abundance of organic germs in different localities. A series of flasks were all one-third filled with the same putrescible fluid (a solution of sugar and albumen was employed in most of the experiments). The fluid was then boiled for 2 or 3 minutes in the flasks and the neck of each flask was drawn out to a fine point and hermetically sealed while the fluid was hot. These flasks were then taken to different localities and the points of the necks were broken and the air of the several localities allowed to rush in and fill the flasks. This violent ingress of air carried in of course all the dust held in suspension and all other principles known or unknown associated with it. In this condition each flask was again hermetically sealed and the whole placed where they were kept at a uniform temperature of 80° to 85° F., a temperature known to be the most favorable for the development of animalcules and mucors. The results of these experiments were not what the principles generally admitted would lead us to expect but they were perfectly consistent with the theory of the diffusion of germs.

Generally in 3 or 4 days the liquid in the flasks was found altered, but in flasks placed in identical conditions were found very different organisms—much more varied so far as mucedines and torulas were concerned than if the liquids had been freely exposed to ordinary air. On the other hand it frequently happened in a series of experiments that several of the flasks remained absolutely unaffected for an indefinite time as if it had received only calcined air.

This simple and unobjectionable method of experimenting appears to demonstrate that the cause of so-called spontaneous generation does not exist in the ambient air throughout its whole extent, but that it is possible to take up in a single place and at a given instant a considerable volume of ordinary air which, without having undergone any physical or chemical change, is altogether unsuitable to give origin to infusoria or mucedines in a liquid which is invariably thus altered when it is exposed to the open air. The partial success of these experiments shows that by these movements of the atmosphere there is always brought to the surface of a putrescible liquid in an open vessel a quantity of air sufficient to furnish germs suitable to be developed in two or three days.

It appears that the organic productions in the flasks are more various than if the contact with the air had been free, i. e., the organisms in the several flasks are different. This result might have been expected, for by limiting the rush of air and repeating it with different flasks, a small number of germs would be collected in a limited portion of air and the growth of these germs would not be obstructed by other germs, more numerous or more vigorous or rapid in their growth, capable of monopolizing the soil to the exclusion of those less vigorous or less rapid in growth.

It was found that the number of negative results varied greatly with the atmospheric conditions, and that nothing was easier than to increase or diminish the relative proportion of flasks which gave birth to the organisms mentioned, or the number in which they were totally absent.

In the cellars of the observatory at Paris, so situated as to have very little change of temperature, and where the air was remarkably quiet, the proportionate number of flasks that were opened in that locality without producing any organisms was much greater than for the same number of flasks opened in the court-yard of the Observatory where the air was constantly agitated.

The explanation of this difference is obvious. Although the air of the cellars of the Observatory, nearly saturated with moisture, was more fitted for the production of the various kinds of mould and infusoria than the open air of the court-yard, yet the stillness of the air in the cellars allowed all ova and spores to be deposited by the force of gravity, and few or none remained floating in the air which rushed into the flasks opened in that locality. In proportion as more precautions were taken to avoid agitation of the air there was less appearance of organization, and Pasteur concludes that if the flasks could be opened and closed in the cellars without the disturbance of the air caused by the entrance of the operator there would be the same absence of vitality in the flasks filled with air from that locality as if they were filled with air exposed to a red heat.

The following results were obtained by Pasteur with flasks opened in widely different localities:

Sixty-three flasks were each one-third filled with a clear liquid obtained by filtering water mingled with the scum of beer, all solid matter being removed by the process of filtering. This liquid is known to be very susceptible of change, for exposure to ordinary air for two or three days is sufficient to give birth to small infusoria or a variety of mucedines. The fluid was boiled in all the flasks and they were hermetically sealed as in the previous experiments. Twenty of the flasks thus prepared were opened and closed in the country far from any habitation, at the foot of those heights which form the first plateau of of the Jura mountains.

Twenty other flasks were filled with air upon one of the mountains of the Jura 850 metres (2789 ft.) above the level of the sea. Another series of twenty flasks were carried to Montanvert near the Mer de Glace to an elevation of 2000 metres (6562 ft.) where they were filled with air and hermetically sealed like the others.

Of the twenty flasks opened in the level country six developed organic productions. Among the twenty flasks opened upon the plateau of the Jura, only five developed organisms. But of the twenty flasks filled with air at Montanvert, when a strong wind was blowing from the gorges of the Glacier des Bois, one only produced organisms.

These experiments show that the air from elevated localities is remarkably free from those germs which give origin to organic products.

In collecting air for these experiments the following precautions were adopted to avoid as far as possible the intervention of dust carried by the operator or deposited on the outside of the flask or other implements required in performing the experiments. The elongated neck of the flask was first heated in the flame of a lamp and a scratch was made upon the glass with a file. The flask was then raised above the head with the end of the neck turned toward the wind and the point was broken off with long iron forceps, the branches of which had previously passed through flame to destroy any dust adhering to their surface so that it might not remain to be driven into the flask by the sudden rush of air when the point of the flask was broken. Great pains were taken lest the agitation of the liquid in the flasks during transit might exert some influence unfavorable to the development of infusoria or mucedines.

The following results are therefore without objection and they show the entire difference between the air of the plain or of elevations and that of inhabited places. Pasteur's first experiment at the Glacier des Bois was interrupted by a circumstance which had not been foreseen. He had taken to close the points of the flasks, after they were filled with air, an eolipile lamp fed with alcohol. The whiteness and glare of the ice, in the light of the sun, was so great that it was impossible to see the jet of alcohol flame, and as it was agitated by the wind it could not be directed upon the glass with sufficient steadiness to melt the point and hermetically seal the flask. As no means were at hand to render the flame visible, the flask could not be sealed, and there remained chances of error by the admixture of other powders. The three flasks which had been opened were therefore taken to the small village of Montanvert and sealed at his lodgings the next morning, after they had been exposed all night to the dust of the chambers where he slept. Of these three flasks only two produced either infusoria or mould. Since the number of flasks altered in this experiment is greater than that in those which followed, (the twenty flasks previously noticed), Pasteur concludes that the agitation of the liquid during the journey had no influence upon the development of germs.

It therefore appears to be satisfactorily demonstrated:

1. That the air of inhabited places contains a greater relative number of fruitful germs than the air of uninhabited regions.

2. That the ordinary air contains only here and there, without any continuity, the condition of the first existence of generations sometimes considered spontaneous. Here, there are germs and there, there are none.

3. There are few or many, according to the localities. Rain diminishes the number but after a succession of fine days they are more numerous. Where the atmosphere has been for a long time quiet germs are wanting and putrefaction does not take place as in ordinary circumstances.

Gay Lussac, Schwann and Pouchet have performed various experiments upon liquids in contact with common air, with heated air, with artificial air, and with oxygen gas, using a mercurial bath to isolate the substances experimented upon. Some of their results have appeared to favor the theory of spontaneous generation. Pasteur has ascertained that mercury taken from the bath in any laboratory is itself loaded with organic germs. He took a globule of mercury surrounded by an atmosphere of calcined air and passed it into a flask of putrescible fluid by the process detailed in the former part of this paper. In every experiment of this kind after two days an abundant growth of organic products appeared.

The same experiments were repeated with the same liquids, with no change of manipulation, with the same kind of mercury, except that the mercury was first heated to destroy the germs it contained, and no growths whatever appeared in the flasks.

From all these experiments Pasteur concludes that: *Powders* suspended in the air are the exclusive origin, the first and necessary condition of life in infusions in putrescible bodies and in liquids capable of undergoing fermentation. It is easy to collect and observe with the microscope atmospheric dust, among which may always be found a great number of organized corpuscles which the experienced naturalist will distinguish as the germs of inferior organisms.

[Some infusoria are not more than $\frac{1}{24000}$ of an inch in diameter and if we suppose that the ova of infusoria and the spores of minute fungi are no more than one-tenth part the linear dimensions of the parent organism there must be an incalculable amount of germs no larger than $\frac{1}{240000}$ or $\frac{1}{100000}$ of an inch in diameter. Since according to Sullivant and Wormley, (This Journal, vol. xxxi, p. 12,) vision, with the most powerful microscope, is limited to objects of about $\frac{1}{80000}$ of an inch, we need not be surprised if infusoria and other organisms appear in putrescible liquids in far greater numbers than the germs in atmospheric dust visible by the aid of the microscope would lead us to expect.—Tr.]

Pasteur proposes to continue these investigations and expresses the hope that the way may thus be opened for a successful investigation of the origin of different diseases.

New Haven, May 1, 1861.

Art. II.—*Natro-boro-calcite and another Borate occurring in the Gypsum of Nova Scotia;* by Henry How, Professor of Chemistry and Nat. Hist., King's College, Windsor, Nova Scotia.

About three years and a half ago I showed the existence of Natro-boro-calcite in the Gypsum of Windsor N. S.* I was not aware at that time that Dr. Hayes of Boston, U. S. had announced his conviction† that the soda which had been attributed to this mineral was an impurity and had given as the true expression of the composition of the pure mineral the formula $CaO\ 2BO_3+6HO$. Had I known this I should have adverted to the probability of his mineral, (Hayesine, *Dana*) constituting a distinct species from Natro-boro-calcite whose existence seems to be sufficiently established by the repeated finding of not very dissimilar quantities of soda in analyses of specimens from two of its three localities, as seen in the following list which contains all the analyses I have been able to find:

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* Edin. N. P. Journal, July, 1857. This Journal, Sept. 1857.

† This Journal, Nov., 1854, p. 95.

	BO_3	CaO	HO	NaO	KO	SO_3	NaCl	Sand	
Peru,	46·11	18·89	35·00	....	....	....	....	....	Hayes.[a]
Tuscany,	51·13	20·85	26·25	....	....	....	....	....	Bechi.[a]
Peru,	49·50	15·90	25·80	8·8	....	....	....	....	Ulex.[b]
Peru,	49·50	17·70	26·00	8·8	....	....	....	....	" [b]
Peru,	45·46	14·32	....	8·22	0·51	1·10	2·65	0·32	Dick.[a]
Peru,	43·70	13·11	35·67	6·67	0·83	....	....	....	Ramm.[c]
Peru,	47·25	15·98	25·46	9·88	....	0·45	....	0·98	Anderson[d]
Nova Scotia	41·97	13·95	34·39	8·36	....	1·29	MgO	0·04	H. How*
" "	44·10	14·20	34·49	7·21	....	....	....	....	"

(*a*) Dana's Min., 4th Ed., p. 394. (*b*) Liebig und Kopp's Jahrb. 1849, p. 780. (*c*) this Jour., Sept., 1857, Third Supp. to Dana's Min., p. 6. (*d*) Proc. Phil. Soc., Glasgow, Feb., 1853.

In the account of the analysis by Anderson the quantities of soda and SO_3 as above given are reversed, but from the conclusion drawn by the author this is evidently a mistake. As regards the amount of water present, no mention is made in any case but my own as to the temperature at which the substance was dried; in my analyses the mineral was air-dried. The soda, it will be observed, is a constant ingredient in pretty uniform amount in all but the first two analyses, and in my examination, as stated at the time, the mineral was washed for the second analysis with cold water till all sulphuric acid was removed.

From the preceding data the following formulas have been deduced:

$CaO\ 2BO_3+6HO$	Hayes,
$NaO\ 2BO_3+2CaO,\ 3BO_3+10HO$	Ulex,
$NaO\ 2BO_3+2CaO,\ 3BO_3+15HO$	H. How,
$NaO\ 2BO_3+2(CaO\ 2BO_3)+18HO$	Rammelsberg,

all referring to a mineral found in rounded masses, consisting of interwoven fibres, opaque, snow-white and of a silky lustre.

The mineral to which I would now draw attention was found in the same quarry as the preceding, at a distance of about 100 yards and at about 20 feet lower level, and also associated with Glauber-salt, which, it is worthy of notice is generally met with here, according to the quarrymen, in narrow seams at the line of junction of the "hard plaster," (Anhydrite) with the "soft plaster," (Gypsum). I detected it in the form of an opaque white substance without lustre, and, to the naked eye, devoid of crystalline structure, in cakes and somewhat rounded masses varying in size from that of a small pea to that of a bean; these masses lay between gypsum and crystals of Glauber-salt, taking shape from the crystals of the latter on the side next to them, and when detached from them leaving their faces, as it were, etched, and sometimes the crystals were penetrated to a considerable depth by the imbedded borate. The mineral is very soft, (H.=1) but coherent, tasteless, slightly tough between the teeth, fuses readily B.B. to a clear bead, insoluble in water, soluble in HCl. As found, or very soon after being brought home, it lost by exposure to air, Water=18·36 per cent, and the air dry substances gave the following results on analysis; the water was determined by ignition, the lime, magnesia and sulphuric acid in one portion of the ignited residue, and the soda in another, after its treatment with fluor-spar and sulphuric acid for separation of boracic acid, which was, of course, estimated by deficiency:

	I.	II.
Lime	14·21	——
Soda	7·25	——
Sulphuric acid	3·98	——
Magnesia	0·62	——
Water	19·96	20·78
Boracic acid	53·98	——
	100·00	

The quantity of mineral obtained did not permit me to make more than one analysis and retain a little as a specimen for identification, but these results as well as the characters already mentioned and the crystalline structure to which I shall presently advert, are, I think, sufficient to show that it is specifically distinct from Natro-boro-calcite (see analyses quoted). On the assumption that the magnesia and sulphuric acid are accidental, and that the latter is combined with the former and with a quantity of soda equivalent to that of the acid not required by the magnesia, I have calculated the preceding results (I) after making these deductions, and at the same time taking away the amount of water necessary to render the $MgOSO_3 = MgOSO_3 + 7$ aq.; (the hydrated sulphate of soda would of course become anhydrous on exposure to dry air); the results then become:

				Calculation.		
		Oxygen.	Ratio.			
Lime,	15·55	= 4·44	3·08	$3CaO$	84	15·64
Soda,	5·61	= 1·44	1·00	NaO	31	5·77
Water,	19·72	=17·52	12·16	$12HO$	108	20·11
Boracic acid,	59·10	=40·47	28·10	$9BO_3$	314·1	58·48
	99·98				537·1	100·00

corresponding to the formula,

$$NaO, 3CaO, 9BO_3 + 12HO$$

I am very well aware that it is unsafe to base a formula upon a single analysis, especially of a mineral substance, and most especially after making deductions as above, and I cannot in this case insist on the one brought out, but it is not anomalous. We find rather complex combinations both in the natural and artificially formed compounds of boracic acid, thus:

Hydroboracite,* $= 3CaO, 4BO_3 + 3MgO, 4BO_3 + 18HO$, and
Larderellite,* $= NH_4O, 4BO_3 + 4HO$.

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* See Dana's Min. 4th Ed., pp. 394, 395.

while Laurent* describes a salt $=5NaO, 24BO_3+52HO$, and Rose† one $=3CaO, 5BO_3$ when ignited, and it is a little curious that the formula given above includes the soda salt corresponding to Larderellite and the salt of Rose.

$$NaO, 4BO_3+3CaO, 5BO_3+12aq.$$

I mentioned that the mineral presented no appearance of crystalline structure to the naked eye; not having at hand, at the time I was at work upon it, a sufficiently good microscope, I sent a portion of the mineral to Professor Robb, of the University of New Brunswick, at Frederickton, with a letter stating my results and my doubt as to the substance having crystalline structure; I received this answer; "In spite of your odd formula, the mineral, just as I got it, untouched and unwashed, is perfectly crystalline in every particle. A good power is required, but with a magnifying power of about 350 diameters there is no difficulty, the form comes out as sharp as possible. The crystals are excessively thin translucent tables or plates. They have a rhombic outline and the angles probably =80° or more, owing to their excessive thinness I could not say whether they could be called right or oblique rhombic prisms. I suspect the latter from analogy. By care the 'Tiza' (Natro-boro-calcite) can be shown to consist of very fine prisms, sharp, angular and long, but too fine for me to state their form. The diameter was less than ·00118 of an English inch. The long prismatic needles of the Tiza are in great contrast to the broad tables of the recent mineral in your last letter; of that the plates are about ·0048 of an inch from side to side, but some are a little larger, others a little smaller. In some you see regular cleavage, that is, a small rhomb chipped out of one side. As far as form goes therefore it would seem to be a distict and definite species. I presume it was formed in a dry place for the angles were quite sharp. The connection between these borates and sulphates of lime and sulphate of soda is very curious."

I may state that I had subsequently the opportunity of appreciating the great accuracy of this description of the appearance of the two minerals.

Arguing from the chemical composition, which however may not be quite established, and the crystalline structure, I conceive the mineral in question to constitute a new species, and I propose for it the name of Cryptomorphite (*ϰϱυπτος*, occultus, *μοϱφη*, forma) in allusion to its microscopic crystalline structure.

The truth of the last sentence in Prof. Robb's letter is very apparent. In my former paper on the subject I adverted to the existence of Natro-boro-calcite in the Gypsum here as confirming

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* Liebig und Kopp's Jahresbericht, 1849, p. 226.

† Liebig und Kopp's Jahrb., 1852, p. 313.

Dawson's theory of the origin of the rock from volcanic waters acting on the carbonate of lime; it is interesting to observe that Bechi found[*] the same mineral, with other borates, in the lagoons of Tuscany. The hydrated conditions of both the borates found in the rock here and of the associated Glauber salt shows the action of water, but that of ordinary sea-water would not account for the presence of boracic acid. As regards the soda, the sulphate and borate of lime were probably the substances originally present, and chlorid of sodium in water being introduced might remove part of the calcium as chlorid, and furnish borate and sulphate of soda; it is confirmatory of this view that a small quantity of rock-salt in crystalline grains has lately been found in the Gypsum.

ART. III.—*On Gyrolite occurring with Calcite in Apophyllite in the Trap of the Bay of Fundy;* by HENRY HOW, Professor of Chemistry and Nat. Hist. King's College, Windsor, Nova Scotia.

THE Mineral Gyrolite was first described by Professor Anderson of Glasgow,[†] as a new species from the Isle of Skye; it is stated by Greg and Lettsum[‡] to occur without doubt at two localities in Greenland, and, according to Heddle, at Faröe. The only other notice of it that I am acquainted with is by L. Sæmann, who[§] mentions that he examined a specimen, no locality being given, mixed or interlaminated with pectolite, and suggests that this mineral losing its alkali becomes gyrolite, and losing its lime becomes okenite. No other analysis than the original one of Professor Anderson has, I believe, been published; the following account of its occurrence among the minerals of Nova Scotia, shows it in such association as affords a mode of explaining its origin by change in apophyllite. I met with it in Anapolis County, N. S., some 25 miles S. W. of Cape Blomidon, between Margaretville and Port George, on the surface of fractured crystalline apophyllite, and, on further breaking the mass a good many spherical concretions of pearly lustrous plates were observed in the interior, of sizes varying from that of a pin's head to nearly half-an-inch in diameter; their outline was well defined and the external characters as given by Anderson were recognined on examination; it afforded the following results on analysis. The mineral was ignited for water, and the residue treated with HCl, the resulting dried silica was weighed, and then fused with carbonated alkali, and the weight of the small quantities of alumina, etc., so separated, was deducted from that of the first silica. I place my numbers by the side of those of Anderson, and give the calculated percentages for his formula:

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[*] Dana's Min., 4th Ed., 394, 395.

[†] Trans. Roy. Soc. Edin. and Phil. Mag., Feb., 1851.

[‡] Manual of Min., p. 217.

[§] First Supp. to Dana's Min., p. 9. This Jour., May, 1855.

	How.	Anderson.	Calculation.		
Potassa,	1·60	....			
Magnesia	0·08	0·18			
Alumina,	1·27	1·48			
Lime,	29·95	33·24	32·26	$2CaO$	= 56
Silica,	51·90	50·70	52·18	$2SiO_3$	= 90·6
Water,	15·05	14·18	15·55	$3HO$	= 27·0
	99·85	99·78	99·99		173·6

and a general accordance is observed sufficient to show the identity of chemical composition in the minerals examined; the small quantity of potassa present in my specimen probably modified the blowpipe character a little as I found it not to exfoliate completely, and it fused without any difficulty, and even with some ebullition.

Some of the numerous cavities in the apophyllite were empty, some entirely filled with gyrolite, and in others separate plates of this mineral were standing edgewise, leaving vacant spaces, while upon and by the side of the plates were in some cases rhombohedral crystals which proved to consist of calcite and were sometimes present alone in the cavities, which varied from being quite shallow to half an inch in depth. It is mentioned by Anderson that gyrolite occurs associated with stilbite, laumontite and other zeolites, and is sometimes found coating crystals of apophyllite.

The difference in chemical composition between apophyllite and gyrolite is very well seen on comparing the respective theoretical percentages of their constituents, thus:

	SiO_3	CaO	KO	HO
Apophylite,	= 52·70	26·00	4·40	16·70+HF variable.
Gyrolite,	= 52·18	32·26		15·50

and the existence of the calcite in the cavities seems clearly to show that the gyrolite is formed from the apophyllite by the waters which deposited the carbonate of lime reacting on the silicate of potass and dissolving out at the same time the fluorine as fluorid of calcium:* trial was made for fluorine on two fragments of the gyrolite and no evidence of its existence obtained.

* See Dana's Min. I, p. 232–233.

ART. IV.—*On some questions concerning the Coal formations of the United States;* by LEO LESQUEREUX.—(Continued from vol. xxx, 384.)

General Remarks on the Coal Plants and their Study.

IN the memoirs of the Geological Survey of Great Britain, Dr. Hooker has remarked at length on the difficulties attending the study of the fossil plants of the Coal measures.[*] I can admit, indeed, with the celebrated English author that this study is far more difficult than could be supposed from an examination of some specimens of fossil ferns, which are found sometimes so handsomely painted upon the shales, that they look as perfect as plants preserved in the herbarium. But I cannot share the whole of his views concerning the insufficiency of the data furnished by palæontological botany and their uncertainty compared with those afforded to Geology by fossil animal remains. Remains of fossil plants are preserved in two ways:

First. We find, especially in the shales overlying coal-banks or occupying their place, the flattened surface, the printed outlines of some peculiar vegetable organs. They are mostly leaflets of ferns, either without traces of fructification, or with the fructification obliterated and dimly visible through the parenchyma of the leaves;[†] or parts of fronds, broken pinnæ detached from the common rootstock; or pieces of flattened stems without leaves, generally bearing on the surface some peculiar striæ or cicatrices left at the point of attachment of the leaves; or a few isolated and broken fruits, apparently nutlets of unknown relation and structure.

Secondly. We find also fossil botanical remains, either silicified or transformed into coal or mineral charcoal. Silicified wood is common enough in some strata of sandstone intervening between some beds of coal. But it represents parts of stems, or of half decayed trunks, of which the bark has generally been taken off or is entirely obliterated. Such specimens of course expose to the student the internal structure of the wood but nothing else. When the remains of fossil plants are found preserved in coal or charcoal, every trace of a complex organism has disappeared, and nothing can be seen by a microscopical examination but isolated vessels of various forms and size.

We have thus either flattened organs of plants of which we can only see the outline, to which we cannot by any means refer the organs that are essential for determination, like stems, flowers, and fruits, &c., and of which we cannot study the internal structure. Or we have isolated vessels transformed into coal or pieces of silicified wood of which we can study the internal structure only without possibility of comparing it or of referring it to external organs so as to know the outward appearance of the vegetable to which they belong.*

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[*] It is well to recall the fact that the remarks in this paper regard only the fossil coal plants, though some of them may apply to the fossil flora in general.

[†] The lower surface of the ferns which bears the fruit, is mostly the one inseparably attached to the stone.

To show at once in its most unfavorable and discouraging aspect the difficulty of studying the botanical palæontology of the coal, we have still to remark that the broken, separated organs of fossil plants, especially the leaflets of the ferns, generally found detached from the stems, are extremely variable. Some perfectly alike in their outline may belong to different species while some others apparently totally different belong to the same plant or even to the same pinna.

As a compensation to these apparently insurmountable obstacles, we have:

1st. The extraordinary simplicity of the flora of the coal and the small number of species that appear to have contributed to its formation. All the leaves found in connection with the coal measures belong to ferns and to a few genera of unknown affinity.

2d. As every botanist well knows, most of the species of ferns have a peculiar and well marked nervation. In the fossil ferns of the coal this nervation is generally well preserved, visible and characteristic enough for the identification of species.†

3d. In some rare cases where the nervation is nearly alike for different species, each of these may be distinguished by peculiar marks, hairs, scales, tubercles or appendages, &c., which permit their identification.

4th. Moreover, one may truly say, that each species even in its most different parts has a peculiar look, easily known at first sight by the palæontologist, though indeed indescribable. And this is true as well for the leaves of the ferns as for the cicatrices which so peculiarly mark the bark of some trunks.

From this it follows, that the characters on which we have to rely for a determination of the fossil plants of the coal are uncertain, indeed, and of little value for an absolute classification or in comparison with all the species heretofore known, but that they are generally reliable enough for identifying the species of coal plants relatively to each other. This, I think is all that is wanted for an examination of the palæontological botany of the coal. For the flora of the coal-measures constitutes a peculiar group of plants which ought to be studied by itself, whose characters ought to be looked for without any exact relation to plants now living. A certain amount of botanical knowledge, especially of the anatomy and of the geographical distribution of the plants of our epoch, is necessary for that duty; but it can be pursued without the assistance of great and costly botanical cabinets, and even without the acquaintance with a large number of our living plants. Of course the more one knows of botanical science, the more one is prepared to judge of the analogy of the forms of fossil plants or to recall them to an original type. But nobody will attempt the study of fossil plants who has not been prompted by a natural love of the science to devote himself to serious botanical studies.

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* Brongniart, Hooker, Corda, Göppert and a few other palæontologists have had the opportunity of studying through thin, polished sections, the internal structure of a few species of fossil wood, and cones from specimens still preserving the outward form of their bark. They could thus compare the anatomical structure with some external characters. But the pretence of identifying species or even genera from the characters of isolated vessels appears inadmissible, considering the great likeness of these elementary organs in different species and also their variety according to the age of the stem and to their place within it. This question will be examined in detail hereafter.

† Though the number of species of ferns now living is very great, the nervation is still considered by some botanists as sufficiently characteristic to separate the species of a genus and even of subgenera. (See the opinion of Mittenius in this Journal, vol. 31, p. 183).

Comparing now the data furnished by both animal and botanical remains, and considering what is called the insufficiency of botanical palæontology, we have to inquire, first, What results are to be expected from palæontological researches? We reply, nothing less than some more or less precise indications concerning the succession of the beings which have inhabited our globe since the first appearance of life, or a kind of history of the creations or of the modifications of what we may call the *forms of life.* And, as a corollary of the first proposition, some indications of the changes to which the surface of our globe has been subjected, and which, according to the laws governing the forms of life, ought to be attended by the appearance of peculiar beings.

It is indisputable that the external forms of some animals, their shells especially, are generally better preserved in a petrified state than the soft parts of most plants. But animal remains, bones, scales, teeth, are only isolated parts of a whole, and are no better applicable to an exact scientific classification or to a satisfactory identification of a species than a single leaf of a tree. As for the shells, they are simply envelopes, and their affinities to the animal are at best only imperfectly understood. It is true that comparative anatomy has done more for animals than for plants. But the number of leaves is not small of which the outline is so closely related to the form of the whole vegetable that when they are found in a fossil state, they may be immediately and certainly referred to their proper genus. For what concerns families of extinct and unknown forms, I think that the reconstructions offered by comparative anatomy are as reliable for *Lepidodendron*, *Sigillaria*, for whole forests indeed rebuilt from pieces of bark or from leaves as they can be for whole fishes, and gigantic animals rebuilt from a scale or a bone.

From another point of view, if it is true that the number of animal fossil remains is far greater than that of fossil plants, it cannot be denied that animal remains are mostly marine and are cast, accordingly, in a scarcely variable general mould. If we should judge of the successive developments of the vegetation of our globe by the fossil marine plants only, we should, indeed, come to a very erroneous conclusion; the *Fucoides*, for example, of all the formations are much alike, and preserve even up to our time their original typical characters.

Marine animal remains appear to have been subjected to appreciable changes only by great geological events, of which natural philosophy cannot thus far fully appreciate the amount of influences. Thus, of course, we have not any solid ground from which to draw reliable conclusions concerning the result of those influences upon the forms of animal life. Botanical palæontology has engraved its records on the rocks, only at very distant epochs, and they present only broken links of a chain, which science may perhaps never be able to connect.

From this it appears that when we come to examine the succession of species from their origin, and search for arguments concerning the problematical question of successive changes or of successive creations, the data now furnished by animal and botanical palæontology respectively, give us about the same amount of light and merit the same reliance.

As indicating the succession of the strata of our globe, or as geological marks, animal remains have this advantage: that as most of the strata have been formed by marine deposition, these remains are more generally found in every part of the geological measures. They are thus an ever present if not always a reliable guide. But the most interesting part of the geological field is entirely barren of those *medals of the creation* to which we look for a record not only of the successive deposition of strata but of all the changes which the surface of our earth has undergone. Marine strata have been formed from materials transported and mostly, at least, taken from dry land. Touching this dry land, the multiplicity of changes to which it has been subjected by that potent and most variable agent, the atmosphere; concerning the various appearances of the successive stories built by the Eternal Power for the ultimate end of the structure, viz:—the habitation of man; about some of the materials prepared and heaped up for his welfare and perfect development; concerning the great harmony of all the beings in the different ages, marine remains cannot give much information. Thus, if the data furnished by one of these special branches of palæontology are more numerous, more universal, more easily found and better preserved and studied—those of the other are more precise and so to speak, more applicable to our humanity. Because they speak a human language. They speak of atmospherical changes, of sun and of rain, of seasons and of their variations. They speak of shores battered by the waves and covered with floating debris, of hills, of green fields, of impenetrable forests, of everything that is now in the fullness of its perfection, belonging to the Eden slowly prepared for our abode.

If this does not prove that fossil botany is as useful to science and as interesting as any other branch of palæontology, the few words I have said in its defense will easily be excused: especially as in this country it has had few followers, and is often looked upon with unmerited discredit.

By far the greatest obstacle to the study of the fossil plants of our coal measures is the difficulty of obtaining good specimens, and, when the specimens have been found, of procuring books to make us acquainted with them. We have indeed some few specimens of fossil plants in geological cabinets. But they have been mostly collected by persons unacquainted with fossil botany and are thus incomplete in many ways and of little use for study. Either they want some part essential for a determination, or they have been collected without a close regard to local and stratigraphical position. Fossil botany ought to be studied in the coal mines, among heaps of shales where remains of the same species can be collected in their different forms. Sometimes hundreds of pieces of the shales have to be split before the part of the frond of a fern which may be desired for a satisfactory determination can be found. In many fossil ferns, especially in the *Neuropterideæ*, the terminal leaflet of the pinnæ has a peculiar and characteristic form; and, as the leaflets of this class of ferns are mostly deciduous, these terminal ones are rare and difficult to find. Though whole strata of shales are covered with leaflets of *Neuropteris hirsuta*, and though this species is found at nearly every geological horizon, it is nearly impossible to find the branches with leaves attached to them, and thus to know something of the general appearance of this species.* With the pieces of the bark of some trees, viz. with specimens of *Lepidodendron*, *Sigillaria*, &c., the difficulty of collecting is still greater. For the points or cicatrices left by the leaves, and generally showing in peculiar forms the deposition of the vessels at their point of emerging from the stems, vary in size and distance according to the age of the vegetable or the place on the trunk from which the specimen is taken. Moreover, the cicatrices have various forms following the process of decortication of the vegetable. As it is extremely rare to find in our coal measures a whole tree preserved by petrification, the study of a single species of the great vegetables of the coal ought to be pursued at the same place on a great number of specimens. This is always a difficult task. But if we consider that before we are able to become well acquainted with any species of fossil plant, and especially to get a view of its stratigraphical and geographical distribution, we have to examine it closely at a great number of distant exposures, the difficulties of this study appear far greater.

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* I have only two small specimens of this species with branches and leaves; one found by myself the other presented to me. Mr. Bunbury saw for the first time such a specimen in the cabinet of Mr. Brown of Nova Scotia.

The collecting of specimens and their study would be greatly encouraged by some good work with descriptions and figures of American fossil plants. Indeed, I have been repeatedly asked what were the books from which we could get that preliminary acquaintance of fossil plants which is necessary to direct such researches. Hitherto nothing has been done in that line in the United States; at least nothing that can be easily obtained by the student, because all that has been published on the botanical palæontology of the coal is disseminated in scientific Journals or State Geological Reports that are not generally accessible.

Before the publication of the Geological report of the State of Pennsylvania, a few species of our fossil plants had been described by Mr. Brongniart in his *Fossil Flora** from specimens sent to him, chiefly by Prof. Silliman, from Wilkesbarre and Zanesville. After this, Mr. Bunbury of England published in the Quarterly Journal of the Geological Society of London (vol. ii, p. 82) some remarks on species of fossil plants collected by Mr. Lyell at Frostburg, Maryland. Very few of these species are described or even determined, for except *Neuropteris cordata*, *Pecopteris arborescens*, *Lepidodendron tetragonum*, *Lepidodendron aculeatum*, *Stigmaria ficoides*, *Asterophyllites* and *Calamites nodosus*, all the species mentioned in Mr. Bunbury's paper are marked with signs of doubt. The general remarks of this celebrated English author are very interesting indeed, but are useless for one beginning the study of the fossil plants of our coal measures. Even the three new species which he has described and figured are uncertain, being made from incomplete specimens. In the third volume of the same Journal, Mr. Bunbury, examining some fossil plants from the Nova Scotia coal measures, sent to him by Mr. Richard Brown, mentions, mostly without descriptions, and with a ?, forty-eight species, of which seven are described and figured. The same remark as above can be applied to these species, except for *Lepidodendron tumidum* and *Neuropteris rarinervis*, satisfactorily described and figured, though still from imperfect specimens. This Journal, vol. iii, No. 1, contains a *Notice of vegetable impressions* by Granger,

* *Histoire des Vegetaux Fossiles.*

who has given some figures, but without names and scientific descriptions. This Journal, vol. xxix, 1st series, contains an excellent and elaborate article by Dr. Hildreth of Marietta.* A number of our common species of fossil plants are figured in the plates accompanying this paper; but the names and the descriptions of the plants are not given. The remarkable observations of Steinhaur in the *Transactions of the American Phil. Soc. of Philadelphia*, (vol. i, new series) apply to plants observed and examined in the coal basin of England; and those of Harlan in the same journal (1831) concern only some *Fucoides* and have no relation to the coal. These are all the materials which were available for studying the American fossil plants of the coal when I began the examination of the fossil flora of Pennsylvania in connection with the Geological survey of that state. The Report made on this subject contains the description of two hundred and thirty-one species of fossil plants, with figures of one hundred and two of them, mostly new. This number certainly embraces the greater part of the fossil plants of our coal measures. But the report made at the time with all the care and the light which the collected materials could afford is still defective in many points.† Some species considered and published as new, had been described and published previously; and some others supposed identical with European species, are now acknowledged as distinct. Moreover this report, like those numerous geological reports published among us at the cost of the States, is not on sale and can be found only in certain public libraries, and in the hands of a few privileged men of science.

About the same time that this report was made, Dr. Newberry of Cleveland, published in the Annals of Science of Cleveland, a series of papers, the two first of which (Feb. 1st, and Feb. 15th, 1853) contain a catalogue of one hundred and twenty-seven species of fossil plants of which twenty-two are marked with a ? or without name. In the same Journal (March 1st, May 1st, May 15th, 1853, and Jan. 1854), the same author gives a description with figures of a few species, especially fruits. These papers are now difficult to obtain.

To complete the list of what has been published on the fossil plants of this continent, I have to mention still, some very interesting papers on species of coal plants in Nova Scotia, and on the traces of vegetable organs found in the coal, by Prof. J. W. Dawson of Montreal. The same author has enumerated in his *Acadian Geology* about one hundred species of fossil plants belonging to the coal fields of Nova Scotia. Lately Mr. Horatio C. Wood has furnished to the Proceedings of the Academy of Natural Science of Philadelphia (June, 1860,) a paper containing descriptions and figures of some species of *Sigillaria* and *Lepidodendron.*

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* *Observations on the Bituminous Coal deposits of the Muskingum Valley, &c.*

† This Journal, July, 1860, vol. xxx, p. 64, note.

If in addition to these local papers scattered in periodical works I mention a catalogue of fossil plants published by the writer for the Scientific Society of Pottsville, Penn., I think that we have here the whole amount of contributions to the fossil flora of our coal measures of America.

The larger number of books on fossil plants have been published in Germany and mostly in the German language. In the beginning of this century, Schlotheim published his *Flora der Vorwelt,* with fourteen plates.* Since then science has made such progress that, with the exception of the figures which have been copied by subsequent authors, this work, very remarkable indeed at the time of its publication, is scarcely of any value to the student. From 1821 to 1838 Count Sternberg labored and gave to the scientific world a *Versuch einer geognostich botanischen Darstellung der Flora des Vorwelt*† in two folio volumes. This work contains a great number of good figures of fossil plants which cannot be found elsewhere, and is indeed very valuable for the great amount of information that it contains. It bears the mark of its early origin, and traces of uncertainty by a close and excellent observer who had to find his path in a new field, and give an account of things that had never been seen before and that have no relation to what we have now on hand for a comparison, in the vegetation of our world. *Sternberg's Versuch* was made with great labor and expense, and with the assistance of two of the best German Palæontologists: Corda, who prepared sketches for comparative phytotomy of the fossil and recent plants;‡ and Presl, who elaborated the greater part of the second volume of the book.

Corda, who has been just mentioned in connection with Sternberg, published afterwards his observations in a separate work, *Beitrage zur flora des Vorwelt.* This book is of great scientific interest for the study of the anatomical structure of the wood of some species of fossil trees. A few of the conclusions of the celebrated German author concerning the affinity of stems of the coal with plants of our time are indeed subjected to controversy, but his work in patience and detail of execution is truly admirable. It is still the best guide to consult for those who have opportunity to study the anatomical fossil botany from ground and polished lamellæ of silicified wood.

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* I mention only the most interesting of the works published in Europe on the fossil botany of the coal, and especially those that can be purchased.

† There is a translation of this work in French.

‡ *Skizzen zur Vergleichenden Phytotomie vor und jetztwettlichen Pflanzen Stämme.*

Prof. H. B. Göppert of Breslau has worked immensely upon the fossil flora of the coal. But unhappily some of his memoirs are disseminated in scientific journals. The most important of them which can be got by purchase are, 1st, the *Systema Filicum Fossilium* (1836), descriptions in Latin, explanation in German, and good plates: the *Gattungen fossilien Pflanzen*, (six livraisons have been published), with text both in German and in French. The work is splendidly illustrated; the *Flora des Uebergangs gebirges,* Latin and German: the *Monographie der Fossilen Coniferen,* (Leiden, 1850), and the *Fossile Flora des Silurischen, Devonian*, &c., Leopold Academy, Jena, 1860. This last I have not seen yet, but it is considered as very valuable. It is certain that no living man knows more about the fossil flora of the coal and perhaps of all the formations* than Prof. Göppert. Perhaps his writings are only too numerous and his scientific riches too large. His Genera of Fossil Plants is only commenced and the completion of it is to be ardently desired by every student of fossil botany. In connection with Berger, Prof. Göppert has also published a monography (*De fructibus et seminibus in formatione lithanthracum*) of the fruit of the coal measures which is of great interest.

To Prof. F. Unger, who like Göppert has also published many valuable books on the flora of the recent formations, we owe especially a *Synopsis of the genera and species of fossil plants* which, though made without a clear and fixed method, is very useful as containing an account of all the species of fossil plants known up to 1850, the time of its publication. The descriptions (without figures) are insufficient for the beginner.

Prof. H. G. Bronn has published in his *Lethæa Geognostica* a number of fossil plants of the coal.

From Prof. E. T. Germar we have eight livraisons (the last in 1853) of coal plants. The work in folio has very good plates and descriptions, but progresses very slowly.

Gutbier (Aug. v.) published in 1836 a a small work on the fossil plants of the coal of Zwickau. Though it is printed on poor paper and is without pretention to scientific value, this small book contains a great amount of solid information, and will be consulted with pleasure and profit by those who are interested in our coal flora.

The *Versteinerungen der Steinkohlen Formations in Sachsen*, by Prof. Bruno Geinitz (Leipsic, 1855), is a magnificent folio book with splendid tables and excellent figures. It has been made with great care and from the examination and comparison of a great number of specimens. This is probably the best work to give a general view of the fossil flora of the coal. Many of our common American species are found described and figured in it. It is indispensable to the student. Prof. Geinitz seems to have taken a point of view entirely different from that of Göppert and both, I believe, have gone too far; this one by multiplying the genera and the species, without characters always sufficient; the other in uniting into one some species certainly distinct. The work of Geinitz is entirely in German. It is to be regretted that it does not give at least a short Latin description for each species.

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* I would not even make this restriction, if the admirable work of Prof. Heer on the Flora of the tertiary of Europe had not surpassed all that has been published on the fossil plants of the recent formations.

Goldenberg has made a beginning of a *Flora Sarræpontana Fossilis;* two livraisons only of the work have appeared (1855 and 1857). If it is ever finished it will be a very valuable work indeed. It contains already on the fossil *Lycopodiaceæ* (to which the author refers the genus *Lepidodendron*) and on the fossil *Selagineæ* (to which he refers the genera *Sigillaria* and *Stigmaria*) a great amount of acute and very interesting observations. To Goldenberg we owe the discovery of the fruit of Sigillaria and good details of its structure. He even asserts that he has found the fructification of *Stigmaria ficoides*, and describes it in his generic diagnosis. Goldenberg appears to be one of those true *workmen* of science who spend their time in the mines examining specimens, and collecting them, not for their fine appearance, but to compare and unite the dismembered parts of a species in order to reconstruct the whole; the only way of studying botanical palæontology with advantage to science.

We can scarcely be astonished to find in France only a single representative of the science of the fossil flora of the coal, that of Prof. Ad. Brongniart: for this celebrated author has done so much for the Botany of the Coal measures, that truly his works are beyond comparison and imitation. What a pity that his great *Histoire des Vegetaux Fossiles* has not yet been and probably will never be finished! The last part delivered to science was printed, I think, in 1844. When we look to the details of his comparative botanical anatomy, to the admirable clearness of his classification, to the exactness of his descriptions and of the figures, to every part of his great work, we cannot be astonished that every attempt at classification and description of fossil plants is done in imitation of Brongniart's method, the true foundation of the science. His observations on the *structure of Sigillaria elegans* is a work of anatomical study that is equalled only by that of Dr. Hooker's on the structure of the *Lepidostrobi*, in the memoirs of the Geological Survey of Great Britain. Besides these remarkable works, and among numerous memoirs of Brongniart, the *Tableau des Genres des Vegetaux Fossiles*, and the *Prodrome d'une Histoire des Vegetaux Fossiles* will be studied and studied again with constant advantage and pleasure. If the great fossil flora of Brongniart was finished, it would suffice for the study of the coal plants, at least for the general acquaintance so desirable to direct the researches.

From England, we have the *Fossil flora of Great Britain*, by Lindley and Hutton, (3 vols. 8vo). Many species of the coal measures and of the oolitic formations are described and figured in this work, which is certainly very valuable. But it is made without any systematic arrangement and without method. The descriptions are moreover far from being satisfactory and the drawings too artistical or imaginary.

I have already mentioned the remarkable memoir of Dr. Hooker, who is certainly one of the greatest botanists of our time. From polished lamellæ of fossil cones of *Lepidodendron*, he has exemplified the fructification of this genus as clearly and as perfectly as it could have been done from living specimens.*

Artis, *Antediluvian Phytology*, &c., is a good book for the plates, but scarcely of any use now to the student of fossil plants. The same may be said of Mammats, *A Collection of Geological Facts and Practical Observations*, &c. But Witham's work on *the Internal Structure of Fossil vegetables* is, like *Corda's Beyträge*, very valuable to direct the researches of comparative anatomy in the study of the internal structure of the fossil woods. A number of fossil plants of the coal are described and figured in English works on Geology by Lyell, Buckland, Miller, Mantell, &c.

(*To be continued.*)

ART. V.—*On the Production of the Ethyl Bases*; by M. CAREY LEA, Philadelphia.

I MENTIONED in a former number of this Journal that while engaged in the examination of the action of ammonia on certain oxy-ethers, I had met with the experiments of Juncadella and De Clermont, and finding that the production of ethylamine by these reactions had been already indicated, I discontinued my investigations. Subsequently however having occasion to prepare a considerable quantity of ethylamine for other examinations, I determined to ascertain whether the action of nitrate of ethyl on ammonia could not be made use of as a convenient process. Juncadella had already proposed to mix nitrate of ethyl with three or four parts of alcohol, saturate the mixture with dry ammoniacal gas, and heat for two days to 212° in the water bath. This was a rather troublesome process, and a few experiments led me to the following very simple method, which I publish as having given me satisfactory results:

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* Mr. Lesquereux has apparently overlooked Robert Brown's brief, but very interesting paper, in the *Transactions of the Linnæan Society*, vol. xx, part 3, entitled, *Some Account of an Undescribed Fossil Fruit*, which, in a single specimen, completely demonstrates the structure and affinities of *Lepidostrobus*.—EDS.

Nitrate of ethyl is mixed with an equal volume of strong alcohol. To this mixture is added a quantity of ordinary strong liquid ammonia equal in bulk to that of the mixture. The nitrate of ethyl is thereby precipitated but is nevertheless easily acted upon by the ammoniacal liquid. Three hours in the water bath at 212° is generally sufficient to complete the reaction. As the process goes on the nitrate of ethyl becomes brownish, gradually diminishes and finally disappears. The resulting solution does not contain salts of ammonia and ethylamine only, as has been stated, but *a large quantity of diethylamine and triethylamine.*

As the nitrate of ethyl can be prepared in large quantity with great facility by a modification of M. Millon's process, which will appear with some other papers in the following number of this Journal, I am inclined to think that this process will be found valuable for the preparation of the ethyl bases. It is necessary that the tubes should be strong, and not more than two-thirds full. Bottles hermetically sealed are not to be recommended unless extremely strong. In one experiment I operated with four eight ounce bottles all of which stood the test, but on repeating the process three of the four exploded at once and the fourth subsequently.

The presence of alcohol is far from being essential. Nitrate of ethyl is readily decomposed at 212° by aqueous ammonia, and if the ammonia is in considerable excess, the decomposition is complete in three hours. If the excess is only slight the time required for decomposition is greatly increased. In one experiment where 29 parts of nitrate of ethyl were heated with 57 of liquid ammonia, the decomposition was not complete till at the end of 15 hours. The use of alcohol and the proportions above given are what was found to give the most satisfactory result.

Philadelphia, April 9, 1861.

ART. VI.—*On the Exact Separation of the Ethyl Bases;* by M. CAREY LEA, Philadelphia.

IT has been known for some time that when ammonia was acted upon by the halogen ethers, the ethylamine produced was always accompanied by a variable quantity of the secondary and tertiary ethyl bases and the ammonium base, and I have shown that the same is the case with respect to the secondary and tertiary ethyl bases in the reactions of oxy-ethers. The ammonium base I have not looked for, but it is doubtless present also. The primary, secondary and tertiary bases resemble each other so closely in their properties that their separation is attended with great difficulty and has generally been effected by the different solubilities of their chloroplatinates.* I find however that it may be accomplished with great ease by means of picric acid. The picrates of the different bases exhibit the most opposite degrees of solubility, the picrate of triethylamine rivalling the picrate of potash in insolubility, while that of diethylamine is soluble to an almost unlimited extent in water, alcohol and ether, without being in the least deliquescent. Its affinity for ether is so great that its concentrated etherial solution will remain exposed to the atmosphere for days in a syrupy condition, gradually crystallizing on the surface, and eventually all through, to a radiated mass. Between these two extremes the picrate of ethylamine occupies an intermediate position and the differences of solubility are so well marked as to render the separation perfectly easy in the manner to be indicated below.

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* Dr. Hoffman has lately described an elegant mode of separation, differing however, entirely from that here proposed, viz. by means of oxalic ether.

1. *Separation of the mixed bases from Ammonia.*

This has been generally directed to be done by treating the mixed hydrochlorates either with absolute alcohol, or with a mixture of strong alcohol and ether, whereby the salammoniac is supposed to be left undissolved.

This process I have always found to the last degree unsatistory, and necessarily so; for if absolute alcohol be allowed to stand over salammoniac in powder for a time and then be evaporated in a watch glass, a certain quantity of salammoniac will be deposited and the same is true of a mixture of strong alcohol and ether. By operating on the ammonias in the form of sulphates a much more satisfactory result is obtained. For this purpose the mixture as poured out from the pressure tube is treated with a sufficient quantity of pure sulphuric acid to displace the nitric acid (or if bromid or iodid of ethyl have been employed, the bromhydric or iodhydric acid) and the solution is evaporated as far as possible at a temperature of about 250° F. The pasty mass is exhausted with strong alcohol, the alcoholic solution is evaporated as far as possible at the same temperature and the residue exhausted with a small quantity of absolute alcohol. Two exhaustions at least, are necessary even when absolute alcohol is used the first time. In this way a satisfactory result is obtained, which cannot be accomplished with the chlorhydrates.

2. *Separation of the mixed bases from each other.*

TRIETHYLAMINE.

After being completely freed from ammonia as above described, the mixed sulphates were distilled with caustic potash, in the usual way and the bases were saturated with crystallized picric acid. Sufficient heat was applied to redissolve the precipitate which formed toward the close of the saturation, and the hot (not too concentrated) solution allowed to crystallize. The first crop of crystals thus obtained were purified by several re-crystallizations, the chlorhydrate of the base was formed and from this the chlorplatinate which was obtained in large and beautiful crystals. The platinum salt was analyzed.

	Per cent.
·4858 gms. substance gave ·1567 platinum,	
This corresponds to	32·26
Chloroplatinate of triethylamine contains	32·23

The close correspondence of the result with the number given by theory indicates the perfect purity of the triethylamine. Its proportion is but inconsiderable compared with that of ethylamine and diethylamine, and it is chiefly contained in the first portion of the gaseous products which pass over in the distillation with caustic potash.

Picrate of triethylamine crystallizes in delicate yellow needles, which dissolve sparingly in cold water and alcohol, abundantly in hot. The decrease of solubility in an aqueous or alcoholic solution as it cools is very sudden, so that a hot solution deposits almost the whole of the salt as it begins to cool. Placed on platinum foil and gently heated, it melts, turns first red, then black, at the same time boiling up, takes fire, and leaves a residue of charcoal.

ETHYLAMINE.

After the least soluble salt had been removed in the manner just described, the mother water deposited another crop of crystals, which were purified by repeated recrystallization, the chlorhydrate of the base was formed, and its chlorplatinate was analyzed.

	Per cent.
·4429 gms. substance gave ·1728 platinum,	
This corresponds to	39·02
The chloroplatinate of ethylamine contains	39·29

Picrate of ethylamine presents great differences of appearance according to accidental circumstances. When first crystallizing out of the mixed solution it usually forms groups of short brown prisms adhering to the bottom of the basin. If these be recrystallized we obtain long yellow flattened prisms and laminæ, extending in every direction through the liquid—few substances exhibit more beautiful crystallization. A dilute alcoholic solution often deposits flat prisms bevelled at the extremities, and sometimes hexagonal plates.

In solubility in water this substance approximates to the picrate of ammonia, to which, after it has been subjected to repeated recrystallizations, it bears considerable resemblance in appearance.

DIETHYLAMINE.

After the picrate of ethylamine has been for the most part removed, the mother waters exhibit a curious phenomenon which greatly simplifies the further purification of the diethylamine salt left in solution. When these mother waters have been a little reduced by spontaneous evaporation or by a gentle heat, the liquid spontaneously separates into two layers, a pale yellow lighter and a dark brown heavier. The evaporation is continued until the heavier layer constitutes rather more than half of the whole, and then the two layers are separated either at once by a separating funnel, or ether may be added and thoroughly shaken up, when the brown layer dissolves in the ether and rises to the top. After separation it is allowed to crystallize and is once or twice recrystallized from a small quantity of ether by which it is obtained quite pure. A specimen was converted into chlorhydrate and then into chlorplatinate, and gave the following results on analysis:

·3884 gms. substance gave ·1377 platinum.	Per cent.
This corresponds to	35·45
chloroplatinate of diethylamine should contain	35·45

It appears therefore that the salt obtained in the above described manner is perfectly pure.

Picrate of Diethylamine without being deliquescent is soluble to almost any extent in water, alcohol and ether. By spontaneous evaporation it crystallizes in a radiated mass. Sometimes at the bottom of the vessel beautiful transparent brown yellow crystals form, which belong to the monoclinic system. Combination observed ∞P∞, [∞P∞], 0P, ∞P, +P, and a clinodiagonal doma.

The approximate proportions in which the different ethyl bases were produced by the reaction here made use of were perhaps ten per cent of triethylamine, thirty or forty of diethylamine and fifty or sixty of ethylamine.

The method here described gives with facility a very exact separation of these bases. The only precaution necessary to be observed, especially in operating on small quantities, is that the solution must not be too concentrated at the outset, otherwise the picrate of triethylamine will be mixed with picrate of ethylamine. But the delicate needles of the former are easily distinguished from the broader ones and prismatic crystals of the latter, and two or three recrystallizations suffice to separate them completely.

I feel assured that picric acid will be found to be a most valuable reagent in effecting separations of cognate organic bases. It may be doubted if there exists any other acid whose salts taken as a whole exhibit so marked a tendency to ready crystallization, and the great differences in the solubility of picrates of allied bases, adds, as in the foregoing example, to the value of this acid in effecting separations between them.

Philadelphia, April 9, 1861.

ART. VII.—*On the Path and Velocity of the Guernsey County (Ohio) Meteor of May 1st*, 1860; by Prof. E. W. EVANS of Marietta College.

IN a brief account of this meteor published in this Journal of Science, July, 1860, I gave the most reliable and definite observations which I had been able to collect, bearing on the question of the meteor's path and velocity; I also gave such conclusions as the data seemed to me to warrant. I propose now to review the subject more at length, in the light of all the facts now in my possession; partly in order to state, in a more careful manner, both my conclusions and the arguments by which they seem to me to be established; and partly in order to correct some serious errors, in regard to the data, which appear in former communications on this subject.

Prof. J. L. Smith, of the University of Louisville, in an article published in the January number (91) of this Journal, begins by summing up "all the observations" which he considers "worthy of note respecting the fall of this meteorite." In this summary the statement is repeatedly made, that the village of New Concord, near which the largest stones fell, is nearly east from the village of Cambridge, at which some of the observations which he records were made:—it is also stated that a large number of stones fell southeast of Cambridge. The truth is that New Concord is nearly west of Cambridge, and that not one of the stones has yet been found to have fallen southeast of the latter place.

On the map contained in Prof. Smith's article, the lines of lattude place the fall of meteoric stones full 60 nautical miles farther north than it really occurred; while Parkersburg, the place of a most important observation quoted by him, is placed too far north by about 37 nautical miles. Such errors, if allowed to stand uncorrected, would involve the whole subject of the meteor's path in confusion.

Among the observations which Prof. Smith selects as noteworthy I find the following. "Mr. D. Mackley of Jackson county states that he was standing on the platform of the railroad station in Berlin, 20 miles south of Parkersburg, when he saw in a northeast direction a ball of fire about 30° above the horizon," &c.

The value of this observation will appear when it is considered that there is no railroad passing through any place 20 miles south of Parkersburg, that there is no place named Berlin in that part of Virginia, and that the village of Berlin from which Mr. Mackley saw the meteor is in the state of Ohio, nearly 50 miles west of the point indicated by Prof. Smith. The quotation is substantially in Mr. Mackley's own words (as reported from the

Cincinnati Commercial by D. M. Johnson, in this Journal, July, 1860), with the exception of the words "20 miles south of Parkersburg," which are added. This mistake is the more unaccountable, because in Mr. Johnson's communication the place of observation is described as 80 miles southwest of Cambridge, while both in Mr. Mackley's letter to the Commercial and in my more complete report of his testimony, from which Prof. Smith elsewhere quotes, the place is precisely designated as Berlin, six miles east of Jackson, Ohio, (vide September number, 1860). But the statement, when corrected, is not of more consequence than several others, which Prof. Smith omits altogether from his list of observations worthy of note; though he afterwards gives them in part, as having been relied upon by Prof. Andrews and myself.

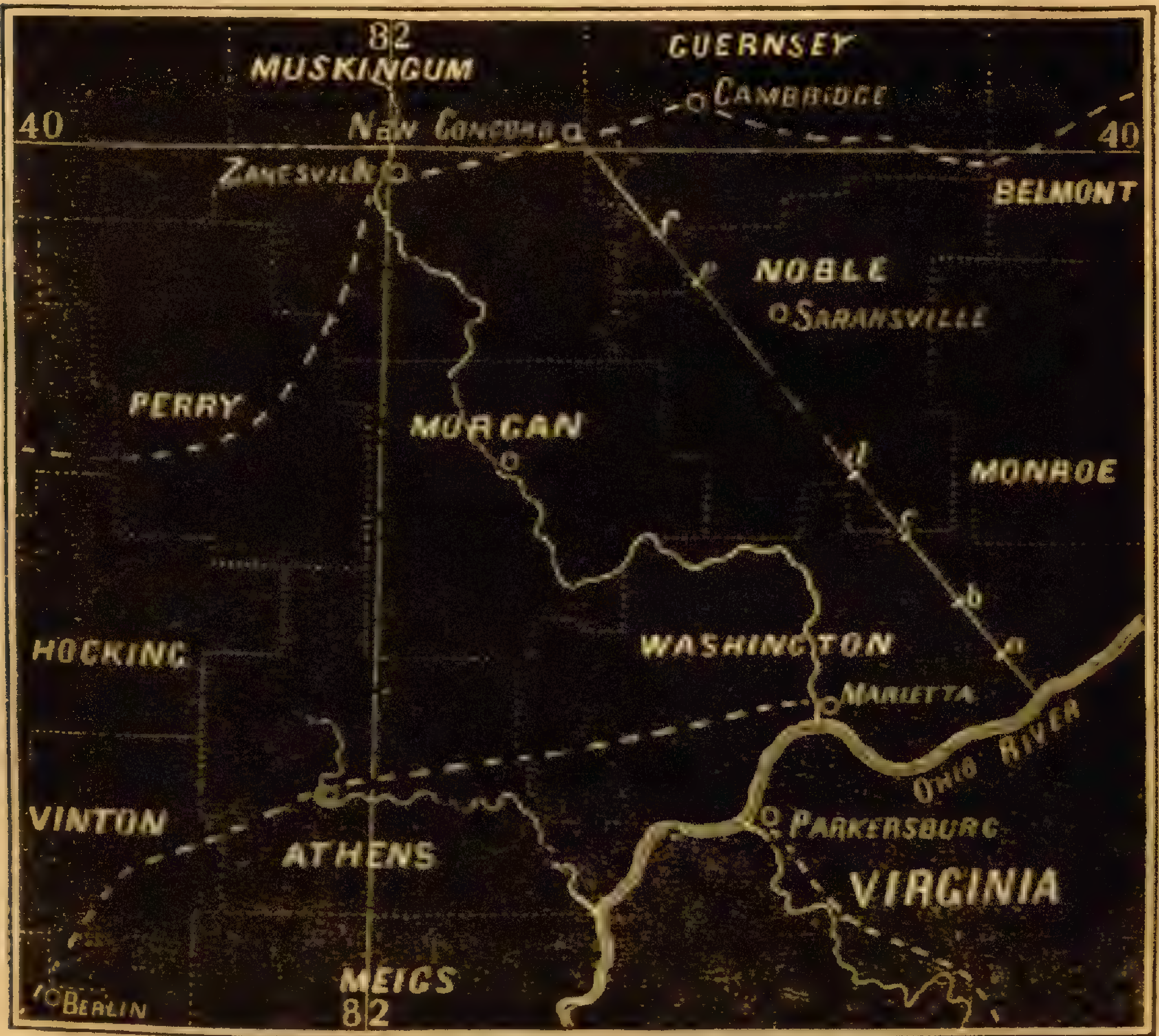
In commenting upon my conclusions, Prof. Smith says:—"as regards the supposed elevation of 40 miles when the first reports were heard, I would simply ask the question, is it possible, with the established views of the conduction of sound by rarefied air, that any conceivable noise produced by a meteorite 40 miles distant from the earth, in a medium quite as rare if not rarer than the best air pump can produce, would reach us at all, or if so, in the manner described by observers?"

I need only say in reply that the writer here attempts to invalidate my conclusions by throwing doubt on premises from which I never reasoned. That the sounds in question were somehow connected with the fall of stones none will deny. That they proceeded from an elevation of 40 miles is a view which might well be received with doubt: it is certainly a view which I never maintained. How the sounds were caused, whether by violent disruption of parts or otherwise, is a question which it would be foreign to the purpose of this article to discuss; but I may state in this connection one important fact relating to them, because I shall have occasion to refer to it again. The successive reports heard at great altitudes in the district where the stones fell, and apparently connected with the descent of the separate pieces through the clouds, were entirely distinct from the one great detonation which was heard at great distances from that district. The former were distinctly heard only over an area of a few miles. The latter shook the buildings from Wheeling, Virginia, to Athens county, Ohio. It is ascertained by careful inquiries to have been heard from Columbiana county on the northeast to within eight miles of Chillicothe on the southwest, and from Knox county on the northwest to the borders of the third tier of counties in Virginia on the southeast: an area of about 150 miles in diameter. At all places within this area, except those near Cambridge and New Concord, it was described as a single sound, a sudden concussion resembling thunder or the discharge of a heavy piece of ordnance, followed by a roar of about two seconds in continuance. A merchant of Marietta, happening to be at dinner, suspected it was the explosion of a powder magazine in his store about a quarter of a mile distant. The Parkersburg News says, "the houses shook as with an earthquake." In the counties of Washington, Morgan, Noble, Monroe and Belmont, and in places along the Virginia side of the Ohio river from Parkersburg to Wheeling, those who were within doors very generally attributed it to an earthquake. The windows rattled; and local papers state that the door of an engine house was jarred open at Bellair near Wheeling. The lines of direction of the sound from all sides, as distinguished by those who happened to be out of doors, cross each other in the southern (not far from the central) part of Noble county; while the inhabitants of that region thought it was overhead. Prof. Andrews, giving the results of personal inquiries, says, "the people of the northern part of Noble county heard it in a southern or southeastern direction, and not in a northwestern direction towards New Concord." At Zanesville about 12 miles from New Concord, the Courier described the noise, not as a succession of sounds, but as an "explosion." These facts clearly indicate that the great detonation heard at these various places was one and the same sound, and that it proceeded from a point over the interior of Noble county. The most probable location is five or six miles south of Sarahsville. It was undoubtedly the *first produced*, but the *last heard*, of the successive sounds described as receding to the southeast by witnesses in the neighborhood where the meteoric stones fell; and it was compared by them to the roar of thunder.

Again, Prof. Smith says, "as regards the size of the meteorite I have but to refer to my experiments made in 1854, and published in the Journal of 1855, to show the perfect fallacy of calculating the size of luminous objects by their apparent discs."

As the above remark is made in reference to my estimate of the size of the meteor, it is but justice to myself to say that I had acknowledged the danger of error from this source, and had only insisted that if the apparent disc and the estimated distance be *assumed* as data, we shall obtain for the diameter of the meteor about three eighths of a mile. (Vide Number 88, July, 1860.)

I may now proceed to the discussion of the meteor's path: and first of all I shall aim to state the data with as much accuracy as possible. It is proper to say that the latitudes and longitudes of places in my first communication on this subject (July, 1860) were inserted by the editors, apparently from common maps. I shall here give latitudes, longitudes, and relative distances of places, as nearly as they can be determined from the most reliable surveys of this part of Ohio yet made; which, so far as the distances are concerned, may be supposed near enough for the purpose in view. The accompanying map is on too reduced a scale to be easily made accurate; but it will aid the reader in understanding the following remarks.



In my former estimates I decided upon that path which seemed to agree best with all the observations then known to me. After more thorough investigation it seems better to give first the results formed from a few observations which there is now reason to consider the most reliable; and then to show how nearly the other observations confirm these.

The witnesses on whom I shall most rely are, Willian C. Welles of Parkersburg, a graduate of Nassau Hall, and D. Mackley,* Esq., a lawyer of Jackson, Ohio. My reasons for this selection are, first the superior intelligence of the witnesses; secondly their favorable places of observation, one at a great distance from the meteor's path and the other comparatively near; and finally the great pains taken by each to note the facts accurately on the spot, with a view to publication. I may add also that I have subjected both of these witnesses to a close examination.

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* This name was misprinted Hackley in July number, 1860.

Mr. Welles' place of observation was in the state of Virginia, (lat. 39° 17′, lon. 81° 24′), about three miles east of Parkersburg. His testimony is as follows. He saw the meteor through an opening in the clouds, first appearing about 50° east of north, and disappearing about 20° east of north. It was in sight about three seconds. Its altitude when 35° east of north was about 65°. Of this he is most confident. When asked at what altitude its visible path produced would cut the meridian to the north of him, he pointed from 50° to 55°. It is important to observe that Mr. Welles' judgment as to angles is to be strongly relied upon, because he is somewhat accustomed to astronomical observations.

Mr. Mackley's place of observation was Berlin (lat. 39° 6′, lon. 82° 23′), about six miles nearly northeast of Jackson, Ohio. His testimony is as follows. He saw a brilliant meteor pass over a cloudless space from about 55° east of north to about 40° east of north. It was moving nearly parallel with the horizon. When it first appeared, its altitude was about 30°; at its disappearance it was about two degrees lower. It was in sight about six seconds. Mr. Mackley's account of the manner in which he estimated the angles serves to strengthen confidence in his accuracy. He says that as nearly as he could judge, the meteor appeared at one third of the distance from the horizon to the zenith, and the arc which it described, when projected on the horizon, would be one half the altitude. He states also that he visited the place again in order to determine, as accurately as possible, the points of the compass.

In order now to make a first approximation, let us assume that the path of the meteor, when projected on the earth, would pass through New Concord, (lat. 40° 1′, lon. 81° 45′), on either side of which the heaviest stones fell. The bearing of this line, as shown by the direction of the route along which the stones were scattered, by the direction in which different pieces are ascertained by Profs. Andrews and Smith to have reached the ground, and by the direction to which the successive reports attending their fall receded, must have been nearly northwest. Let us then suppose, by way of trial, that it was exactly northwest. Mr. Mackley saw the meteor from Berlin in a northeast direction. Now these two directions being at right angles to each other, it follows that its real path was nearly parallel with the earth's surface; for otherwise its apparent path could not, under the given conditions, have been nearly parallel with the horizon, as Mr. Mackley declares it was. It follows also that its height above the earth was not far from 40 miles; for the altitude given by Mr. Mackley is from 28° to 30°, and the distance northeast from

Berlin to the projection of the supposed path upon the earth is about 70 miles.

We may now proceed to correct this first approximation by combining the observations of Messrs. Mackley and Welles. We may assume that the path of the meteor for a short space, such as these two observers saw it traverse, could not have departed very far from a straight line; for it was moving in the highest regions of the atmosphere, and, according to any hypothesis, with immense velocity. Then the line which will best agree with the observations of both, and at the same time, when projected on the earth, pass through New Concord, runs 40° west of north. Let us first consider Mr. Welles' observation: azimuth 35° east of north, altitude 65°. The base line in this case (from Mr. Welles' station to the supposed projection), is 19 miles; the consequent height 41 miles, nearly. This was at a point over the eastern part of Washington county, (*b* on the map). Next take Mr. Mackley's first observation: azimuth 55° east of north, altitude 30°. The base line in this case is 68 miles, and the consequent height (after allowing for the curvature of the earth), 40 miles. This was over the southern part of Noble county, (*d* on the map). Next consider Mr. Mackley's second data: azimuth 40° east of north, altitude 28°. The base line is about 69 miles, and the resulting height 38 miles, nearly. This was over the northern border of Noble county, (*e* on the map). Now by comparing the distances between these stations with the corresponding differences of height, it will be seen that they are not far from proportional; which gives a trajectory between the above limits (from *b* to *e* on the map) not departing far from a straight line, though descending somewhat more in the last part than in the first. But if we suppose the bearing to have been one or more degrees greater or less that 40° west of north, we shall in like manner obtain, from the same observations, a trajectory departing from a straight line altogether too rapidly to be admissible: in the one case, indeed, convex towards the earth; in the other case rising and falling successively within the limits of the atmosphere.

The path now found is consistent with Mr. Welles' approximate estimate of the altitude (from 50° to 55°) at which the arc described by the meteor would, when produced, cut the meridian. In the statements of other witnesses we find as close agreement with those of Messrs. Mackley and Welles as could be expected, from ordinary observers of sudden and startling phenomena. In the neighborhood from eight to ten miles north of Marietta, a considerable number of persons (I mention Jacob Leonhart and two sons, of Bear Creek) caught glimpses of the meteor through the clouds, north and a little west of north, at such altitudes as to show that if its course was nearly northwest, its height was not far from 40 miles over the central and northern parts of Noble county. Many persons on the eastern border of Athens county, west of Marietta, saw the meteor in a northeasterly direction, passing from cloud to cloud at such altitudes as to lead to the same conclusion. Mr. John Brabham and several others undertook to show the angle at which the body was descending towards the horizon, and it was such as to give a path not differing widely from the above, when combined either with Mr. Welles' observation or with that of Mr. Mackley. The statements of different observers at the same places of course vary somewhat; but none have been used except those which seemed well attested. The directions were taken, whenever possible, as pointed out by the observers themselves, from their places of observation. Every case of very wide discrepancy in testimony was by this means made to disappear.

Let us now use the data furnished by Messrs. Welles and Mackley for estimating the velocity of the meteor. It is to be observed that its bearing, as above estimated, being so nearly at right angles with the lines of vision of both observers, reduces the velocity almost to a minimum. Now Mr. Welles saw the meteor pass from 50° east of north to 20° east of north, (from above *a* to above *c*), a distance of 11 miles in about three seconds. This gives for its velocity, in the first part of its visible path, 3⅔ miles in a second. Mr. Mackley estimated that the meteor was visible to him for six seconds. The distance in this case (from above *d* to above *e*) is 18 miles; the consequent velocity three miles a second. Here is as close agreement as could be expected: and in view of the tendency to exaggerate the time, we may presume that neither of these estimates of the velocity is too great; but of the two, that based on Mr. Welles' observation should be preferred, since the shorter interval of time is the easier to estimate with precision.

There is no strong evidence that the meteor was seen further southeast than where it first appeared to Mr. Welles, (*a* on the map), nor further northwest than where it was last seen by Mr. Mackley, (*e* on the map). The distance between these two points, projected on the earth is about 35 miles. In a former communication I gave the testimony of Joel C. Richardson of Warren as tending to show that it was seen over the district where the stones fell; but from a comparison of his statements with those of others in the same neighborhood, I am disposed to admit that he made an error of ten or fifteen degrees in the direction. Rumors of persons in Morgan county having seen the meteor descend nearly to the horizon have, upon investigation, proved groundless.

It was a circumstance very favorable to correct estimates of directions, on the part of observers, that they saw the body through openings in the clouds. From the east side of its path it was not seen at all, as the sky was completely overcast; but no pains have been spared to collect and examine all the observations from the west side, by personal communication with the witnesses.

The conclusions which we have derived from the evidence may now be briefly summed up as follows. The course of the meteor was about 40° west of north. It was first seen over the eastern part of Washington County (about lat. 39° 27′, lon. 81° 8′) at a height of 41 miles, nearly. It was last seen over the northwestern border of Noble county, (about lat. 39° 51′, lon. 81° 34′), at a height of 38 miles, nearly. Its velocity relatively to the surface of the earth, was from 3 to 4 miles a second.*

As the time was half past twelve, noon (May 1st), it follows from the results just given, that its velocity in the solar system was from 20⅓ to 21 miles a second.

As the data cannot be claimed to be more than approximations to the truth, the conclusions cannot. I have given the results found by comparing the data of two excellent observers at advantageous posts, as the most likely to be near approximations. These results agree nearly with my first estimates, (July, 1860†), formed by a general comparison of less select data, before the most material statements in Mr. Mackley's testimony, or any part of the testimony of Mr. Brabham and many others, were yet known to me. Any attempt to establish a path differing widely from that now given, whether in the bearing, or in the height above the earth, or in the amount of departure from parallelism with the earth's surface between the points indicated, will cause the statements, not only of Messrs Mackley and Welles, but of all the observers, to clash hopelessly with each other.

These views are entirely inconsistent with the hypothesis that the whole of the blazing body described by witnesses came to the earth in Guernsey county, (from *f* to New Concord, on the map). If the principal mass fell at all, it must have fallen at a great distance beyond. Whether we suppose it was consumed in the air or passed on, there is no difficulty arising from the fact that it was not seen farther to the northwest; for there is abundant evidence that the sky along its path was overspread with clouds, not only from Northwestern Virginia to New Concord, but to a considerable distance beyond; and I have ascertained from meteorological reports recorded in the Smithsonian Institution that there were clouds, (early in the afternoon, May 1st), over a large part of Northwestern Ohio. Nor is there any difficulty in conceiving how different bodies of the same density, after entering the atmosphere together and moving through it a great distance, could have been so far separated. For the smaller bodies having more surface in proportion to their weight than the larger, (the surfaces being as the squares of the diameters while the solidities are as the cubes), would encounter more resistance from the air. And the smaller bodies, having once fallen below the larger, would receive a still further acceleration to their descent from the increased density of the air; for it is an established fact that, through atmospheric strata of equal depth, the increase of density downwards is by a geometrical ratio. In order however to account for a separation of over 30 miles in a vertical line it is necessary to concede that the part which passed over was much larger than any of those which came to the ground. It must also be conceded that they began to separate long before crossing the Ohio river: a view which is strongly supported by the fact already stated, that over the southern part of Noble county the stones which fell had already descended far enough to cause a concussion in the lower atmosphere that was heard over a vast region.

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* Assuming that these stones are of extra-terrestrial origin, their velocity on *entering the atmosphere* was greater than 4·9 miles per second, the velocity necessary to a circular orbit. Otherwise the major axis of their orbit would be less than 4000 miles, and the orbit traced backward would again enter the atmosphere.—Eds.

† Vol. xxx, 296.

For other particulars of the history of this remarkable fall of stones, and for a chemical analysis of some of the specimens, the reader is referred to this Journal for July, 1860, and January 1861.

Marietta, Ohio, May 1, 1861.

ART. VIII.—*Caricography;* by Prof. C. DEWEY.

(Continued from vol. xxxi, p. 26, Second Series, 1861.)

No. 271. *Carex Norvegica*, Wahl. Schkuhr, Car., fig. 66.

Spica oblonga composita; spiculis 3–5 brevi-oblongis sessilibus androgynis infirne staminiferis remotiusculis sub-densifloris, et infima squamoso-bracteata cum bractea longa-setacea; fructibus *distigmaticis* ovatis vel oblongis ellipsoideis subacutis ore integris apiculatis squama late-ovata subacuta paulo longioribus vel subaequantibus.

Culm 6–10 inches high, erect, leafy below, longer than the leaves; spikelets 3–5, oblong, short, sessile, staminate at the base, a little separated below, a setaceous and rough bract supporting the lowest and rising from an ovate scale; stigmas two; fruit ovate, oblong or ellipsoid, somewhat acute and apiculate with entire orifice, slightly compressed, longer than or equalling the ovate, short, broad and acutish scale.

Long known on the sea-shores of Norway, it was discovered in 1860 at Wells, Me., in marshes, near the ocean, and forwarded to Dr. Sartwell by Rev. J. Blake, thus being first detected in our country.

No. 272. *C. Franklinii*, Boott, in Hook. Fl. Am. Bor., 217, Illust., No. 193.

Spica e spiculis multis diversis fuscis composita; spiculis 6–10, superne aggregatis quatuor vel pluribus sessilibus nunc ovatis oblongis nunc linearibus gracilibus, suprema apice staminifera, proximis toto staminiferis vel non-floriferis, inferioribus supernè staminiferis interdum toto pistilliferis exserto-pedunculatis vaginato-bracteatis nutantibus; fructibus *dis vel tristigmaticis* ovatis oblongis vel ellipticis scabris ore bifidis fuscis, squama oblonga vel ovali scabra fusca acuta paulo longioribus.

Culm sometimes two feet high or more, triquetrous, slender, stiff and erect, leafy towards the base; leaves long and narrow; spikelets very dissimilar, the pistilliferous ovate-oblong or oblong-elliptic, the lower slender pedunculate and nodding, several upper ones sessile and clustered at the summit, the terminal staminate at the apex and near the small and slender-linear staminate or flowerless spikelets; stigmas 2 or 3, as both numbers occur; fruit elliptic or ovate, oblong, compressed, scabrous and tawny with a bifid orifice, with scales ovate or oval, acutish, rough, and nearly equal to the fruit.

Rocky Mountains, *Drummond.*

Resembles C. ovata, *Rudge*, in having stamens above and three stigmas, but differs much, and is strikingly characterized, as is shown by the only specimens from the Rocky Mountains. For C. ovata, see this Journal, vol. x, p. 44, 1826.

No. 273. *C. incurva*, Lightfoot. Schk., Car., fig. 95.

Spica brevi composita; spiculis 5–8 ovatis sessilibus dense-aggregatis squamoso-bracteatis spicam staminiferis capitulum ovatum sub-rotundum flavum formantibus; fructibus *distigmaticis* ovatis convexis tereti-rostratis, squama ovata longè acuta longioribus vel aequantibus; culmo 5–7 unciali saepe incurva.

Culm 5–7 inches high, suberect, often incurved, scarcely longer than the narrow, flat, light-green leaves rising from the base; spikelets 5–7, sessile and ovate, densely clustered into a roundish head half inch long, staminate at their apex; stigmas two; fruit ovate, tapering-rostrate, convex or slightly ventricose, and immature is shorter than the ovate, long-acute scale; light green, except the yellowish head or spike.

High or cold northern parts of Europe; British America, *Dr. Richardson;* Rocky Mountains in Western Nebraska, *Dr. F. V. Hayden.*

Note. The following new species or forms were collected in the "Yellow Stone and Missouri Exploring Expedition" of the United States, in the northwestern part of Nebraska Territory, under the command of W. F. Raynolds, Capt. Topographical Engineers, during 1859 and 1860, and are published by authority of the Secretary of War; Dr. F. V. Hayden, Geologist and Naturalist of the Expedition.

No. 274. *C. Raynoldsii*, Dew.

Spicis distinctis; spica staminifera solitaria terminali ovata brevi-subconica subsessili densiflora squamis oblongis obtusis vel subacutis rufis obtecta; pistilliferis 3–5 oblongis ovatis vel brevi-cylindraceis crassis densifloris, superiore 1–2 sessili brevi parva et vicina, ternis inferioribus plus vel minus pedunculatis foliaceo-bracteatis vix vaginatis, infima longo-pedunculata subremota et declinata; fructibus *tristigmaticis* rotundo-oblongis ellipsoideis obtusè triquetris nervatis apicem nigris brevi-apiculatis ore subbilobis, squama lata ovata brevi-acuta vel submucronata nigro-fusca duplo longioribus vel paulo superantibus; culmo 20-unciali laevi subtriquetro basin foliaceo et vaginato, foliis crassis planis nervatis longiore; foliis forma juniore flaccidis et omnibus laevibus.

Culm about 20 inches high, erect, stiff and smooth, leafy towards the base, and bracteate under the lower spikes; leaves flat, curved, stiff in maturity, but flaccid earlier, slightly rough on the upper part of the edges and shorter than the culm; staminate spikes single, ovate, short-conic; pistillate spikes 3–5, ovate-oblong or short-cylindric, thick and close-flowered; when 4–5, the upper 1–2 spikes short, small and near the staminate, the three lower pedunculate, leafy bracteate, scarcely vaginate, the lower longer and more remote, and the lowest long, pedunculate and some declined; stigmas three and short; fruit oblong, ellipsoidal, round, triquetrous, ovate or sub-cylindric, nerved, black at the short opening apex, orifice sometimes bilabiate, as long as or near twice as long as the broad-short-ovate short acute scale or sub-mucronate tawny-black scale.

Pierre's Hole, valley of Snake River, June 20, 1860, 6000 ft. alt.; and Henry's Fork, June 22, 1860, 5500 ft. alt.—*Dr. F. V. Hayden.*

A distinct and fine species, not to be confounded with either C. nigra, *All*, which has androgynous spikes staminate *above*, or with C. atrata, *Lin.*, which has spike staminate *below*, as well as different fruit; for the last see this Journal, vol. x, p. 271, 1826.

No. 275. *C. vallicola*, Dew.

Spica composita oblonga ½–1 unciali; spiculis 4–7 ovatis sessilibus brevibus parvisque apicem staminiferis nunc approximatis nunc remotiusculis, inferiore squamaceo-bracteata; fructibus *distigmaticis* obovatis substipitatis convexo-concavis brevi-rostratis ore obliquis enervatis, squama lata ovata subacuta dorso fusca margine albo-membranacea paulo longioribus vel subaequantibus; culmo gracili foliis angustis longiore. Interdum spicis nullo flores pistilliferos habentibus, forte non floriferis.

Culm 6–12 inches high, slender, erect, triquetrous, scabrous and naked above, narrow-leafy towards the base; spikelets 4–7, ovate and sessile, near or more remote below, at the apex staminate with close ovate scales on a short cylinder terminating above and especially the upper ones; the lower spikelets scaly-bracteate; stigmas 2; fruit obovate, tapering below, rostrate and stiped, at the orifice oblique, equalling or a little exceeding the broad-ovate subacute scale which has a tawny back and white membranous margin.

Jackson's Hole, on Snake River, June 18, 1860, 6000 ft. alt.—*Dr. F. V. Hayden.*

Differs from others of this family in having the staminate part of the spikelet a short projecting column or cylinder at the apex, often longer than the pistillate part, and having its closely appressed ovate scales.

No. 276. *C. Douglasii*, Boott, Hook, Fl. Am. Bor., tab. 214.

The common form of this species, as found by Dr. Hayden in Nebraska Ter., a few years since, was described in this Journal, vol. xxiv, p. 46, 1857. In 1860 he collected specimens, as described by Dr. Boott, towards *a foot high, with spikelets androgynous and staminate at the apex, with others wholly staminiferous;* and also another form *wholly pistilliferous* and so different in its compound spike as to be ranked as a very clear variety, if indeed it belong to this species. It is here described as

Var. *densi-spicata*, Dewey.

Dioica; spica staminifera e spiculis 5–8 ovatis aggregatis composita cum squamis lato-ovatis oblongis obtusiusculis castaneis margine albis; spica composita omnino pistillifera 1–1½ unciali et semi-unciali-lata crassa; spiculis 15–20 vel pluribus ovatis ellipsoideis, superne singulis et arcte congestis, inferne paulo remotis ramosis vel divisis et arctis, densifloris fructibus *distigmaticis* ovato-lanceolatis rostratis bifidis (immaturis), squama lanceolata castanea dorso viridi margine alba paulo brevioribus; culmo 3–5 unciali erecto laevi rigido, foliis planis vaginatis subradicalibus.

Culm 3–5 inches high, smooth, rigid, erect and shorter than the flat, sheathed and subradical leaves, and wholly pistilliferous; spike 1–1½ inch long and half inch broad, thick, composed of 15–20 ovate spikelets densely aggregated above, and below less near and subdivided or branched, all forming a compact, long head; stigmas two; fruit lanceolate-rostrate, slightly rough on the beak (immature), orifice oblique, bifid, shorter than the lanceolate scale.

Jackson's Hole, on Snake River, or Lewis' Fork of the Columbia, on gravelly knolls, June 15, 1860, 6000 ft. alt.—*Dr. F. V. Hayden.*

Differs from C. Nuttallii, Dew., (Sill. Journ., vol. xliii, 1842,) in its spike and inflorescence, and its relatively broad leaves, as well as its fruit being much less convex and ventricose.

Art. IX.—*Remarks on the Mesozoic Red Sandstone of the Atlantic Slope, and notice of the Discovery of a Bone Bed therein, at Phœnixville, Penn.;* by Charles M. Wheatley, M.A.

[Read before the Connecticut Academy of Arts and Sciences, Feb. 20, 1861.]

No question in American geology seems more difficult of elucidation, than the age and geological position of the so-called "New Red Sandstone" of the Atlantic slope; some geologists referring it to the Oolitic or Liassic periods, others to the Trias, and others still lower, to the Permian. The true position may probably be determined like the San Casciano beds, intermediate between the Triassic and Liassic periods forming a separate group containing like those beds, its own peculiar fossils. No true Permian forms—characteristic of that formation have yet been discovered; the fishes formerly referred to Palæoniscus, are now placed in the genera Catopterus, *Redfield,* and Ischypterus, *Egerton*, their tails being more homocercal than heterocercal. The Clepsisaurus, *Lea*, once considered a Thecodont Saurian and analogous to *Thecodontosaurus antiquus* of Riley and Stutchbury from Redland, near Bristol, England, (found in dolomitic conglomerate referred to the Permian, but now considered not older than the Triassic,) is stated by Dr. Leidy (Proc. Acad. Nat. Sci. Phila., 9 June, 1857,) to be "not properly a Thecodont reptile but may form the type of a new species as its teeth are inserted in the jaws by solid conical fangs." The whole formation is moreover destitute of beds of rock salt and gypsum which characterize mineralogically the Permian system, not only in Russia but wherever recognized. Sir R. I. Murchison (Quar. Jour. Geol. Soc., Lond., vol. i, p. 82,) says, "The Triassic system does not contain a single Palæozoic form whether animal or vegetable whilst the fauna and flora of the Permian are both so connected with the Carboniferous and inferior systems, that they evidently constitute the last remnant of the same era. In the whole geological series, therefore, no two systems are more completely separated than the Permian and the Trias, the one forming the uppermost Palæozoic stage, the other the base of the Secondary deposits."

Prof. Henry D. Rogers in his final Report on the Geology of Pennsylvania, vol. ii, part 2, p. 695, says, under the head "Organic Remains of Main Red Sandstone belt of Atlantic slope," "Reptiles,"—"the main formation or that which alone passes across Pennsylvania, has thus far disclosed the remains of several interesting species: two of these the *Clepsisaurus Pennsylvanicus* and *C. Leai, were first discovered as already intimated in Pennsylvania:* and a species, probably the first named, has been since recognized in the Deep River coal field at North Carolina nearly a prolongation of the same, by Dr. Emmons, who has added several other species of reptiles as belonging to the deposit in North Carolina, namely *Rutiodon Carolinensis*, *Palæosaurus Carolinensis*, and *Palæosaurus sulcatus.* In New Jersey the formation has disclosed the remains of another reptile of the same general structure as the Clepsisaurus; it has been named by Mr. Lea who discovered it, the *Centemodon sulcatus.*"

Prof. Rogers has mistaken the localities, the only Saurian bones discovered in Pennsylvania at the date of his remarks were, vertebra, ribs, and teeth found in the calcareous conglomerate near Hassacs Creek, upper Milford Township, Lehigh county, by Dr. I. Y. Shelley, who presented them to the Academy of Natural Sciences, Phila., Nov., 1847, and upon which Mr. Lea founded his *Clepsisaurus Pennsylvanicus*, (Journal Acad. Nat. Sci. Phil., Part 3, 1853, page 185, &c.); a visit to the locality by Mr. Lea "with Dr. Shelley failed to discover the smallest indication of further specimens," and until my discovery of the bone bed at Phœnixville in October, 1860, these were the only Saurian bones found in the State, with the exception of a portion of a rib sent by the writer to Mr. Lea, noticed in Proc. Acad. Nat. Sci. Phil., 2 June, 1857.

Clepsisaurus Leai Emmons, (*Omosaurus perplexus* Leidy) American Geology, part 6, page 81, fig. 51, mentioned by Prof. Rogers as *first discovered in Pennsylvania, has never yet been recognized in this State,* but was described by Prof. Emmons from Saurian bones found in the Dan River formation near Leaksville, North Carolina. Prof. Leidy thinks Omosaurus probably a distinct genus from Clepsisaurus. Prof. Emmons gives Omosaurus perplexus, *Leidy*, as a synonym of both Clepsisaurus Leai, and Rutiodon Carolinensis.

Centemodon sulcatus, Lea, stated by Prof. Rogers as discovered by Mr. Lea in *New Jersey*, was described from a single tooth, found by Mr. Lea in the black bituminous shales of *Phœnixville Tunnel, Pennsylvania*, (Proc. Acad. Nat. Sci. Phila., viii, p. 77, March, 1856), no other remains of Centemodon have as yet been described.

The following fossils have been noticed in the "Mesozoic Red Sandstone" of Pennsylvania:

PLANTS.

Equisetum columnare, Brong., 15 to 16 inches long, and 7 inches circumference in sandstone of a dark grey color, with iron pyrites, Phœnixville.

Pterozamites longifolius, Emmons, in grey micaceous sandstone, with iron pyrites, Phœnixville.

Gymnocaulus alternatus, Emmons, in light micaceous sandstone, Phœnixville.

Fir-Cones, 6 in. long, 1 in. wide, Isaac Lea, this Jour., [2], vol. xxii, p. 123, 1856, in black bituminous shales, Phœnixville.

Plant resembling that fig. by Emmons, as *Calamites punctatus* in black bituminous shales, Phœnixville.

Plant resembling Noeggerathia at Gwynned, I. Lea (Am. Jour. of Sci., vol. xxii, 1856, p. 123,) probably same as fig. by Emmons (N. Car. Rep. pl. 1, fig. 3,) as Dictyocaulus-striatus, and which Prof. O. Heer (this Journal, [2], vol. xxiv, p. 428,) says "has an obvious resemblance to Noeggerathia." Leo Lesquereux says the genus Noeggerathia, Göpp., entirely disappears at or before the beginning of the coal epoch, (this Jour., [2], vol. xxx, p. 380.)

A number of plants, seed vessels, &c., have been found in the grey micaceous sandstone and black shales, at Phœnixville, the genera of which are yet undetermined.

CRUSTACEA.

Estheria ovata, (Posidonia ovata, *Lea.*)

Estheria parva, (Posidonia parva, *Lea*,) in black bituminous shales, Phœnixville.

Cypris, two species, one smooth, the other beautifully granulate, in black shales, Phœnixville, Rogers, also at Gwynned, J. Leidy, (Proc. Acad. Nat. Sci. Phil., 16 June, 1857).

Limulus? Fragment of Shield probably Limulus, black bituminous shales, Phœnixville, other remains probably Crustacean have been found in black shales, Phœnixville.

MOLLUSCA.

Myacites Pennsylvanicus, Conrad, Proc. Acad. Nat. Sci. Phil., 1857, p. 166, and 1860, plate 1, fig. 3.

FISHES.

Single ganoid scale, in black bituminous shales, at Gwynned, Isaac Lea, this Jour., [2], vol. xxii, 123, 1856, "more like *Pygopteris mandibularis* Ag., than any other which had come under Mr. Lea's notice."

Scales, bones and teeth of ganoid fishes are abundant in black bituminous shales at Phœnixville. Scales have been found by Dr. Leidy also at Gwynned, Proc. Acad. Nat. Sci. Phil., 9 June 1857.

Turseodus acutus, Leidy, Proc. Acad. Nat. Sci. Phil., June, 1857, page 167, "This genus and species are founded upon a left dental bone with teeth, probably of a ganoid fish which I obtained from the black shale of what have been usually considered the Triassic rocks from near Phœnixville, Chester Co., Pa. The dental bone is 20 lines long, by 4 lines in depth; posteriorly, it is straight, and its outer surface is covered with fine, interrupted ridges, such as are observed upon small ganoid scales, found in the same series of rocks at Gwynned."

"Upon the dental border of the specimen there may be counted the remains of 20 teeth, situated at irregular intervals, they have measured from ½ to 1 line long, they are columnar in form, slightly curving inward; have a spreading base, and an abrupt, conical, enamel summit, the fish may be allied to Belonostomus, or Eugnathus, but I am unable to ascertain the exact form of the teeth in these genera."

Radiolepis speciosus, Emmons. *Family Cœlacanthi*, scale discovered at Gwynned, by Isaac Lea, in black bituminous shales, Proc. Acad. Nat. Sci. Phil., 7 July, 1857, also at Phœnixville.

Catopterus gracilis, Redfield. Scales, bones and teeth similar to those from Richmond, Va., and North Carolina, are found in bituminous shales at Phœnixville.

REPTILIAN REMAINS.

Clepsisaurus Pennsylvanicus, Lea, Journal, Acad. Nat. Sci. Phil., new series, vol. ii, 1853, p. 185, founded on vertebra, ribs and teeth discovered in calcareous conglomerate, upper Milford Township, Lehigh county, teeth supposed to belong to this reptile have been discovered by Dr. Leidy in black bituminous shales at Phœnixville, Proc. Acad. Nat. Sci. Philad., 1859, p. 110.

Eurydorus serridens, Leidy, Proc. Acad. Nat. Sci. Phil., 1859, p. 110, founded on tooth "large size, compressed, conical, opposite acute serrulated borders," discovered by Prof. Leidy in black bituminous shales, Phœnixville.

Composaurus —— ? Leidy, Proc. Acad. Nat. Sci. Phil., 1859, p. 110, founded on tooth discovered by Prof. J. Leidy in black bituminous shales at Phœnixville, "borders without serrulations, base fluted" "resembles the teeth of Composaurus of the coal of Chatham Co., North Carolina, but nevertheless belongs to a different species."

Centemodon sulcatus, Lea, Proc. Acad. Nat. Sci. Phil., vol. viii, p. 77, March, 1856, founded on a single tooth discovered by Mr. Lea in black bituminous shales at Phœnixville, described in this Journal, [2], vol. xxii, p. 123.

Bones and teeth probably Batrachian, found by Dr. Leidy at Gwynned, Proc. Acad. Nat. Sci. Phil., 16 June, 1857, in black bituminous shales, also at Phœnixville.

REPTILIAN REMAINS.

Coprolites, very abundant in black bituminous shales at Phœnixville, some of them containing fish remains.

Foot-Tracks, Chelichnus Wymanianus, *Lea*, on dull red limestone? Phœnixville, Isaac Lea, Proc. Acad. Nat. Sci. Phil., viii, 77, 1856.

Ripple marks are also found in the red shale, Montgomery county, opposite Phœnixville.

Section of Strata at Phœnixville Tunnel, Penn., beginning at Eastern entrance, and running about two-thirds through, dip Northwest.

	ft.	in.
Red shale,	5	
Green shale,	6	
Black bituminous shale, containing Saurian bones, coprolites in abundance, Estherias,—remains of ganoid fishes, and Cypris, there are clay concretions about one inch in thickness on upper part, this layer is *full of fossils*.	1	10
Red and green shales, the green slightly calcareous, with traces of Estherias and iron pyrites.	11	
Black bituminous shale with scales of ganoid fishes, Estherias and Cypris, fossils not very abundant.	1	
Dark green, hard, compact shale, full of clay concretions, traces of Cypris.		9
Red micaceous sandstone,	7	10
Brown sandstone, with calcite veins and quartz crystals,	3	6
Hard, compact, red and green shale, with nodular concretions of Limonite, abundantly distributed all through it, forming a "*Hæmatitic conglomerate*,"	5	8
Red sandstone, with remains of plants,	5	6
Red and green shale,	5	2
Red shale, with coprolites and plants, the coprolites inclosing scales of ganoid fishes.		10
Grey sandstone with veins of carbonate of lime,	5	5
Fine-grained, red and green variegated shale,	24	0
Black bituminous shale, with Estherias and fish remains in upper part,	6	
Grey compact fine-grained shale,	11	
Olive green shale, with red veins,	1	
Red shale,	7	
Clay concretions in three layers, 1 inch each,		3
Sandstone with veins of dolomite and calcite—in cleavage which is quite vertical,	11	3
Fine-grained micaceous sandstone, estimated,	20	
Fine-grained compact do do	25	
'Vug' or cavity 5 feet wide at bottom of Tunnel, 21 feet high running to a point about two feet above the back of Tunnel, filled with red and green shales, talcose and micaceous *crushed to powder*,		
White talcose shale vertical, 5 feet wide at bottom, 4 ft. at top of Tunnel.		
Red shale fine-grained, compact,	6	8
Strata very irregular for some distance,		
Shale with clay concretions and oxyd of iron,		10
Bone bed, full of Saurian bones, no other fossils noticed,		6
Black bituminous shale with Estherias and coprolites,		6
Fine-grained, hard, compact sandstone, full of stems of plants,	6	

The "bone bed" is situated about 100 feet in the Tunnel from the western end, and is not more than 6 inches thick. Fragments of Saurian bones occur rather abundantly all through the layer, but the more perfect bones are found at the bottom of the bed where they are collected together forming from two to three inches of the layer, a seam of white or pink carbonate of lime underlies them and is from $\frac{1}{8}$ to $\frac{1}{2}$ inch in thickness. Under this is a very thin seam of black carbonaceous matter, which is grooved and polished like slickensides, evidently showing great disturbing force since the deposition of the bed.

The material composing the bone bed is formed almost entirely of the remains of *Cypris*. No Estherias, Myacites, coprolites or fish-remains, have been observed associated with the Saurian bones in many tons of the shale carefully broken up and examined.

Above the bone bed is about 6 inches of bituminous shale with Estherias and coprolites, over this from 5 to 6 feet of hard, fine-grained sandstone with plants. The bed is underlaid by ten inches of shale with clay concretions which are mostly geodes containing yellow pulverulent oxyd of iron, and under this a compact, fine-grained red shale from six to seven feet to the bottom of the Tunnel.

Near the above in a micaceous dolomitic sandstone of a light grey color, occasionally so calcareous as to effervesce freely in acids, occur Saurian bones—and part of a *jaw* seven inches in length, $\frac{7}{10}$ in. wide, and about $\frac{8}{10}$ in. deep, with seven alveoles about $\frac{3}{10}$ of an inch apart, a *cranial plate* radiated and sculptured $1\frac{1}{2}$ inches long and $1\frac{2}{10}$ in. broad, and an *Icthyodorulite* 3 inches long $\frac{3}{10}$ in. wide at base, remains probably of Batrachians, Estherias, bones, scales, and teeth of ganoid fishes, the scales are large, thick, beautifully ornamented, and coated with a layer of transparent (ganoin) enamel.

Casts of two shells, one may probably be referred to either Pholadomya or Cardita, and the other to Unio or Potamomya and also large quantities of *Saurian teeth*, some of which are full $1\frac{1}{2}$ inches in length, curved, smooth, or finely striated, probably belonging to *Clepsisaurus Pennsylvanicus*, Lea, others curved and sulcate, answering to the description of *Centemodon sulcatus*, Lea. Another perhaps may be *Composaurus*, Leidy, and another of "large size, compressed, conical, with opposite acute serrulated borders" which doubtless is that described by Prof. Leidy as *Eurydorus serridens*. These teeth are found twenty or thirty together and are well preserved, sometimes the teeth are converted into iron pyrites for one half their length, or the pulp cavity alone filled with pyrites, and occasionally small seams of dolomite, calcite, or sulphuret of iron, cross them transversely without disturbing their position. It is remarkable that while the black bituminous shales have afforded but few Saurian teeth, and none have as yet been discovered in the "bone bed," so many should have been collected together and deposited in this strata of dolomitic sandstone as to give it the appearance of an osseous conglomerate or bone breccia.

In some instances *the casts* only of the teeth remain, the substance of the tooth being converted into *dolomite* but retaining the exact form of the tooth with the sulcations as distinct as in the original, twenty teeth of probably three or four genera of Saurians, *all converted into dolomite!* occur on a piece of sandstone 6 by 3 inches. It is a singular fact, that while the teeth are dolomitic casts only, the bones in the same stone remain unchanged, retaining their original structure.

Associated with the above fossils in the sandstone are numerous plant remains, mostly of a broad sulcated stem without joints or branches, as far as noticed they retain the same width their entire length, and are from one half to two inches broad and from six to eight inches long.

The shales, sandstones, and fossils of the Phœnixville Tunnel bear a remarkable resemblance to those of Nagpur and Mangali, Central India, described by Messrs. Hislop and Hunter, Quarterly Journal Geological Society, London, vol. x, p. 472, and vol. xi, p. 371, and referred by them to the lower Jurassic age. The following is the descending order of the series according to the observations of the authors:

1. Soft ferruginous sandstone, sometimes hard, with iron bands, and plants.

2. Fine and coarse argillaceous sandstones, rich, with plant remains, these have afforded:—
   - Labyrinthodont reptile, Brachyops-laticeps, Owen.
   - Fishes, ganoid scales, and small jaws.
   - Crustaceans, Estheria.
   - Plant remains.
   - Fruits and seeds, numerous and undescribed.
   - Leaves, Conifer, Zamites, Poacites and Ferns, (Pecopteris, Glossopteris, Tœniopteris, Cyclopteris, Sphenopteris).
   - Stems, exogenous and endogenous.
   - Acrogens, Aphyllum, Equisetites, Phyllotheca, Vertebraria.

3. Red shales 50 feet, green shales, 30 feet. In the former of these there were observed at Korhádi:—
   - Reptilian foot tracks.
   - Worm tracks, and intestine shaped evacuations, these were also found in the *green shales.*
   - Phyllotheca ?

4. White and colored dolomitic limestones.
   - Bituminous shales with fossils.
   - Sandstone.
   - Indurated clay stone.
   - Green shale.
   - Bituminous shale with fossils.

The plant-bearing sandstone of Phœnixville Tunnel, though not containing all the genera of plants found in the Mangali strata, is far richer in Saurian remains. Crustaceans (Estherias and Cypris) parts of ganoid fishes, and shells. The green shales of the Tunnel have worm tracks, and the intestine shaped evacuations. The bituminous shales are rich in organic remains. The remains of Conifers, Zamites, Equisetites and probably fruits and seeds, with dolomitic sandstones, indicate a very great similarity with the lower Jurassic Central Indian formation.

Phœnixville, Penn., Feb. 1861.

ART. X.—*On the Classification of Organic Substances by Series;*

by JAMES SCHIEL, of St. Louis.

THE progressive series which in 1842* I introduced into organic chemistry, have become the chief means of classification and of connecting organic substances generally. Several years after the introduction of the series, Gerhardt used them in the sense just mentioned in his Traité de Chimie and thereby contributed a great deal toward their general knowledge.† But the idea this distinguished chemist had formed of series was very imperfect, as is proved by the single fact that he ranged benzoic acid and acetic acid in what he called an isologous series (*Traité*, T. I, 127). As in the present state of chemical science, it is highly important that the principles of seriation be clearly understood, I will try to expound them in a more general and methodical manner than has been done hitherto.

The general formula of organic compounds containing carbon, hydrogen and oxygen is $Є_{\alpha}H_{\beta}\Theta_{\gamma}$. By giving to γ successively the values 1, 2, 3, 4, ... we may form the following series of oxydation:

$$\text{(A)}\quad \begin{array}{l} Є_{\alpha}H_{\beta}\Theta \\ Є_{\alpha}H_{\beta}\Theta_{2} \\ Є_{\alpha}H_{\beta}\Theta_{3} \\ Є_{\alpha}H_{\beta}\Theta_{4} \\ \vdots \end{array}$$

From this all the series necessary for the classification of organic substances may be derived by putting $\alpha=n$ and $\beta=2n+2$, making β successively to decrease, the decrement being 2, 4, 6, 8,

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* Annal. Liebig and Wöhler, July number, 1842.

† Gerhardt changed the name progressive into homologous, in the discovery itself of the series he has no share.

. . . By this we have the following series:

$$\begin{array}{llll}
\text{I. } Є_nH_{2n+2}\Theta & \text{II. } Є_nH_{2n}\Theta & \text{III. } Є_nH_{2n-2}\Theta & \text{IV. } Є_nH_{2n-4}\Theta \\
\quad Є_nH_{2n+2}\Theta_2 & \quad Є_nH_{2n}\Theta_2 & \quad Є_nH_{2n-2}\Theta_2 & \quad Є_nH_{2n-4}\Theta_2 \\
\quad Є_nH_{2n+2}\Theta_3 & \quad Є_nH_{2n}\Theta_3 & \quad Є_nH_{2n-2}\Theta_3 & \quad Є_nH_{2n-4}\Theta_3 \\
\quad \vdots & \quad \vdots & \quad \vdots & \quad \vdots
\end{array}$$

and so forth.

Every general formula in one of these generic series represents special homologous series the members of which have the difference $nЄH_2$. When comparing the above series with the hydrocarburets known to exist in a free state, they will be found to represent different states of oxydation of these hydrocarburets.

Another kind of series is formed by making in the general formula $\alpha=n$ and $\beta=n$ and making β successively to decrease or increase, the increment or decrement being 1, 2, 3, 4, ... By this we form the series:

$$\begin{array}{ll}
\text{(B.)} & Є_nH_n\Theta_\gamma \\
 & Є_nH_{n\pm1}\Theta_\gamma \\
 & Є_nH_{n\pm2}\Theta_\gamma \\
 & Є_nH_{n\pm3}\Theta_\gamma \\
 & \quad \vdots
\end{array}$$

Every general formula here represents a series, the members of which are distinguished by $mЄH$ and which I therefore call *hemilogous* series. It is to be remarked that a hemilogous series whose general formula is $Є_nH_{n\pm1}\Theta_\gamma$, can only exist when n is an odd number, as the number of atoms of hydrogen entering into an organic compound is always an even number.* The hemilogous series are very useful for comparing the physical properties of organic substances.

I will now apply these principles of classification to three large classes or groups of substances, the hydrocarburets, the alcohols and the acids. The generic series formed by the first of these groups is the following:

$Є_nH_{2n+2}$ hydrurets of the radicals of the alcohols;
$Є_nH_{2n}$ the homologues of ethylen and the radicals of the alcohols;
$Є_nH_{2n-2}$ acetylen $n=2$; allyl $n=6$;
$Є_nH_{2n-4}$ thymen $n=10$;
$Є_nH_{2n-6}$ benzol $n=6$, toluol $n=7$, xylol $n=8$, cumol $n=9$, cymol $n=10$.
$\vdots$
$Є_nH_{2n-1}$ stilben $n=14$;
$Є_nH_{2n-20}$ hydrocarburet found in the tar of Archangel $n=19$.

The maximum or minimum number of atoms of hydrogen which may be combined with n atoms of carbon cannot be determined at present, but there is no hydrocarburet known to contain more than $(2n+2)$ and less than $2n-(n+2)=n-2$ atoms of hydrogen to n atoms of carbon.

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* If one atom of nitrogen enters into the compound the atoms of hydrogen are an odd number, as nitrogen is triatomic. For this reason alone the formula of Quinine for instance, could not be written $C_{10}H_{12}N\Theta$, as chemists formerly used to do, but $C_{20}H_{24}N_2\Theta_2$ as it is written now for other reasons.

The alcohols form several generic series:

Alcoholic series I.

$Є_nH_{2n+2}Θ$ common alcohols of the type $\left.\begin{matrix}Є_n{}' \; H_{2n+1}\\ H\end{matrix}\right\}Θ$

$Є_nH_{2n+2}Θ_2$ glycolic alcohols of the type $\left.\begin{matrix}Є_n{}'' \; H_{2n}\\ H_2\end{matrix}\right\}Θ_2$

$Є_nH_{2n+2}Θ_3$ glycerine " $\left.\begin{matrix}Є_n{}''' \; H^{2n-1}\\ H_3\end{matrix}\right\}Θ_3$

⋮

whether tetracid alcohols of the general formula $Є_nH_{2n+2}Θ_4$ and the type $\left.\begin{matrix}Є_nH_{2n-2}\\ H_4\end{matrix}\right\}Θ_4$ can exist, is not known, as we know nothing certain about the existence of tetrabasic radicals. The polyethylenic glycols of the general formulas

$$Є_nH_{2n+2}Θ_4$$
$$Є_nH_{2n+2}Θ_5$$

are bibasic alcohols corresponding to the type $\left.\begin{matrix}(Є_nH_{2n})_{m-1}\\ H_2\end{matrix}\right\}Θ_m$.

Alcoholic series II.

$Є_nH_{2n}Θ$ allylic alcohols of the type $\left.\begin{matrix}Є_n{}'H_{2n-1}\\ H\end{matrix}\right\}Θ$

$Є_nH_{2n}Θ_2$ " glycols " $\left.\begin{matrix}Є_n{}''H_{2n-2}\\ H_2\end{matrix}\right\}Θ_2$

$Є_nH_{2n}Θ_3$ " glycerine " $\left.\begin{matrix}Є_n{}'''H_{2n-3}\\ H_3\end{matrix}\right\}Θ_3$

⋮

of this series the only member known as yet is allylic alcohol $Є_3H_6Θ$ corresponding to the general formula $Є_nH_{2n}Θ$.

Alcoholic series III. IV. V.

$Є_nH_{2n-2}Θ$, $Є_nH_{2n-2}Θ_2$, etc.
$Є_nH_{2n-4}Θ$, $Є_nH_{2n-4}Θ_2$, etc.
$Є_nH_{2n-6}Θ$ the homologues of phenylic alcohol;
$Є_nH_{2n-8}Θ$ " cinnamylic alcohol.

The homologous series answering the general formula $Є_nH_{2n-6}Θ$ is

$Є_6H_6Θ$ phenylic alcohol boil. point, 184°.
$Є_7H_8Θ$ benzoylic " " " 204°.
$Є_8H_{10}Θ$ xylenic " " " 220°.
$Є_9H_{12}Θ$?
$Є_{10}H_{14}Θ$ cuminic " " " 243°.

as homologous with phenylic alcohol, the last member of this series, cuminic alcohol, ought to boil at 264°.

Forming a hemilogous series out of such of those alcohols as answer to the general formula $Є_nH_{n+1}Θ$ we have:

$Є_7H_8Θ$ benzoylic alcohol, boil. point, 204°.
$Є_9H_{10}Θ$ cinnamylic " " " 250°.

the difference in composition is here $2ЄH$, the difference in boiling point, 46°; to the difference $ЄH$ in composition there corresponds therefore a difference in the boiling points of 23°, and as to the difference $ЄH_2$ we mostly find a difference of 19°—20° in boiling points, the influence of the atom of H on the boiling point is in this case 3°. In a similar manner the influence of Є on the boiling point may be shown to be 26°, as benzoylic alcohol and cinnamylic alcohol differ by $Є+ЄH_2$ and $ЄH_2$ answers to 20°, we have $46°-20°=26°$. The influence of the single elements on the boiling point may thus be found by seriation and general laws deduced by comparing the different results obtained by this method. The acids form quite a number of generic series.

Series of Acids I.

$Є_nH_{2n}Θ_2$ fatty acids $n=1$ to $n=30$.
$Є_nH_{2n}Θ_3$ glycolic acid $n=2$.
$Є_nH_{2n}Θ_4$ glyceric acid $n=3$ (isomeric with lactic acid.)
⋮

The acids of this generic series correspond to the first alcoholic series.

Series of Acids II.

$Є_nH_{2n-2}Θ_2$
$Є_nH_{2n-2}Θ_3$
$Є_nH_{2n-2}Θ_4$
$Є_nH_{2n-2}Θ_5$
⋮

The special homologous series deriving from this generic series are,

$Є_3H_4Θ_2$ acrylic acid.
$Є_4H_6Θ_2$ crotonic "
$Є_5H_8Θ_2$ angelic "
etc. etc.

$Є_2H_2Θ_4$ oxalic acid.
$Є_3H_4Θ_4$ malonic "
$Є_4H_6Θ_4$ succinic "
$Є_5H_8Θ_4$ lipic "
etc. etc.

$Є_4H_6Θ_6$ tartaric acid.
⋮

$Є_2H_2Θ_3$ glyoxylic acid.
$Є_3H_4Θ_4$ pyroracemic acid.
⋮

$Є_3H_4Θ_5$ tartronic acid.
$Є_4H_6Θ_5$ malic "
⋮

$Є_6H_{10}Θ_8$ mucic acid.

Series of Acids III.

$Є_nH_{2n-4}Θ_2$
$Є_nH_{2n-4}Θ_3$
$Є_nH_{2n-4}Θ_4$
$Є_nH_{2n-4}Θ_5$
⋮

From this generic series are derived:—

$Ꞓ_6H_8\Theta_2$ sorbic acid.

$Ꞓ_4H_4\Theta_4$ maleinic acid.
$Ꞓ_5H_6\Theta_4$ itaconic "
$Ꞓ_6H_8\Theta_4$?
$Ꞓ_7H_{10}\Theta_4$ terebinic "

$Ꞓ_3H_2\Theta_5$ mesoxalic acid.

$Ꞓ_6H_8\Theta_7$ citric acid.

Acid series IV.

$$Ꞓ_nH_{2n-6}\Theta_2$$
$$Ꞓ_nH_{2n-6}\Theta_3$$
$$Ꞓ_nH_{2n-6}\Theta_4$$
$$Ꞓ_nH_{2n-6}\Theta_5$$
$$\vdots$$

From this generic series we derive:—

$Ꞓ_6H_6\Theta_2$ oxyphenic acid.
$Ꞓ_8H_{10}\Theta_2$ terebentilic "

$Ꞓ_4H_2\Theta_4$ mellitic acid.
$Ꞓ_3H_{10}\Theta_4$ cholesteric "

$Ꞓ_5H_4\Theta_3$ pyromucic acid.
$Ꞓ_6H_6\Theta_3$ pyrogallic "
$Ꞓ_7H_8\Theta_3$ ipecacuanic "
$Ꞓ_8H_{10}\Theta_3$ ricinoleic "

$Ꞓ_6H_6\Theta_5$ aconitic acid.*

Acid series V.

$$Ꞓ_nH_{2n-8}\Theta_2$$
$$ꞒH_{2n-8}\Theta_3$$
$$Ꞓ_nH_{2n-8}\Theta_4$$
$$Ꞓ_nH_{2n-8}\Theta_5$$
$$\vdots$$

From this generic series we derive:—

$Ꞓ_7H_6\Theta_2$ benzoic acid.
$Ꞓ_8H_8\Theta_2$ toluylic "
$Ꞓ_9H_{10}\Theta_2$?
$Ꞓ_{10}H_{12}\Theta_2$ cuminic "

$Ꞓ_8H_8\Theta_6$ phenoxacetic acid.

$Ꞓ_5H_2\Theta_5$ krokonic acid.
$Ꞓ_6H_4\Theta_5$ comenic "
$Ꞓ_7H_6\Theta_5$ gallic "
$Ꞓ_{24}H_{40}\Theta_5$ cholic "
$Ꞓ_7H_6\Theta_3$ salicilic acid.
$Ꞓ_8H_8\Theta_3$ anisic "
$Ꞓ_9H_{10}\Theta_3$ phloretinic acid.

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* The radical of the tribasic aconitic acid $\left.\begin{matrix}Ꞓ_6{}'''H_3\Theta_3\\H_3\end{matrix}\right\}\Theta_3$ may be derived from a hydrocarburet by substitution of H_6 by Θ_3, as acetic acid is formed by substitution of H_2 by Θ in the radical $Ꞓ_2H_5$. The triacid alcohol or glycerin corresponding to aconitic acid would therefore be $\left.\begin{matrix}Ꞓ_6H_9\\H_3\end{matrix}\right\}\Theta_3$; it belongs to the alcoholic series II, answering to the general formula $Ꞓ_nH_{2n}\Theta_3$, and the type $\left.\begin{matrix}Ꞓ_n{}'''H_{2n-8}\\H_3\end{matrix}\right\}\Theta_3$. This remark applies, *mutatis mutandis*, to other acids.

$\text{Є}_8H_8\Theta_4$ lecanoric acid. $\text{Є}_9H_{10}\Theta_4$ veratric acid.

Acid series VI.

$$\text{Є}_nH_{2n-10}\Theta_2$$
$$\text{Є}_nH_{2n-10}\Theta_2$$
$$\text{Є}_nH_{2n-10}\Theta_2$$
$$\vdots$$

Of the acids deriving from this generic series only a few are known as yet.

$\text{Є}_9H_9\Theta_3$ cumaric acid. $\text{Є}_7H_4\Theta_7$ mionic acid.

$\text{Є}_8H^6\Theta_4$ phtalic acid.
$\text{Є}_{24}H_{38}\Theta_4$ choloidic "
$\text{Є}_{25}H_{40}\Theta_4$ hyocholic "

From other known acids it is hardly possible to form series, as they stand mostly isolated; there is for instance only one acid —anemonic acid $\text{Є}_{15}H_{14}\Theta_7$—known as yet, which corresponds with the general formula $\text{Є}_nH_{2n-16}\Theta_7$.

The generic series of acids considered above may be ranged into one primitive series:

$$\text{Є}_nH_{2n}\Theta_{2x}$$
$$\text{Є}_nH_{2n-2}\Theta_{2x}$$
$$\text{Є}_nH_{2n-4}\Theta_{2x}$$
$$\text{Є}_nH_{2n-6}\Theta_{2x}$$
$$\text{Є}_nH_{2n-8}\Theta_{2x}$$
$$\text{Є}_nH_{2n-10}\Theta_{2x}$$
$$\vdots$$

where Θ_2^x means, that in order to form a generic series from one of these primitive formulas, Θ has successively to receive suffixed the numbers 2, 3, 4, . . . x. It is easy to see that as every generic series, as for instance:

$$\text{Є}_nH_{2n-2}\Theta_2;\quad \text{Є}_2H_2\Theta_2,\quad \text{Є}_3H_4\Theta_2,\quad \text{Є}_4H_6\Theta_2\ \ldots$$
$$\text{Є}_nH_{2n-2}\Theta_3;\quad \text{Є}_2H_2\Theta_3,\quad \text{Є}_3H_4\Theta_3,\quad \text{Є}_4H_6\Theta_3\ \ldots$$
$$\text{Є}_nH_{2n-2}\Theta_4;\quad \text{Є}_2H_2\Theta_4,\quad \text{Є}_3H_4\Theta_4,\quad \text{Є}_4H_6\Theta_4\ \ldots$$
$$\text{Є}_nH_{2n-2}\Theta_5;\quad \text{Є}_2H_2\Theta_5,\quad \text{Є}_3H_4\Theta_5,\quad \text{Є}_4H_6\Theta_5\ \ldots$$
$$\vdots$$

consists of two kinds of series, of which those lying on the horizontal lines are homologous series, and those lying on the vertical lines are series of oxydation, the primitive series therefore expresses a *seriation in a cube.*

It is worth observing that there may be series of isomeric substances running parallel to each other; such series may be called *homarithmical series.*

ART. XI.—*Theoretical Determination of the Dimensions of Donati's Comet;* by Prof. W. A. NORTON.

(Continued from this Journal, vol. xxix, No. 87, p. 386.)

LET D = the perihelion distance of the comet, and V = the velocity at the perihelion; v = the velocity, θ_1 = the true anomaly, and r_1 = the radius-vector of the comet at any point of its orbit; and β = the inclination of tangent to orbit, to the radius-vector:

τ = the interval of time from assumed date to that of the perihelion passage of the comet:

k' = the acceleration due to the effective repulsion of the sun, expressed in fractional parts of a mile, at the perihelion distance of the comet:

R = the distance from the sun, and Θ_1 = the true parabolic anomaly of the cometary particle, at the instant of its leaving the sphere of influence of the nucleus:

V' = the initial velocity of the particle, in a direction parallel to the tangent to orbit of comet:

Φ = the angle included between the line R and the axis of the hyperbolic orbit of the particle; Δ = the angle included between this axis and the axis of the parabolic orbit of the nucleus; P = perihelion distance, p_2 = the half-parameter, e = the eccentricity, and A = the semi-transverse axis of the hyperbolic orbit; T = the interval of time from assumed date to the instant of the perihelion passage of the receding particle; ψ = the inclination of either asymptote to the axis:

θ = the true anomaly, and r = the radius-vector of the particle at any interval of time, t, after its perihelion passage:

γ = the angle included between the radius-vector of the particle and the axis of the comet's orbit.

Let a and c represent certain constants.

The intervals of time, τ, T, and t, are expressed in days and fractional parts of a day. The velocities V, V', and v, are expressed in miles and fractional parts of a mile, per second.

R and Θ_1 may be taken, with slight error, equal to the values of r_1 and θ_1, found for the same instant of time.

$$\tau = \frac{365^{\mathrm{d}}\!\cdot\!25}{3\pi\sqrt{2}} \,.\, D^{\frac{3}{2}}\left(\text{tang.}^3\,\frac{\theta_1}{2} + 3\ \text{tang.}\,\frac{\theta_1}{2}\right) \quad . \ . \ . \ . \ (10.)$$

$$r_1 = \frac{D}{\cos^2\frac{\theta_1}{2}}; \qquad V' = V\sqrt{\frac{D}{r_1}}; \qquad \beta = \frac{180^\circ - \theta_1}{2} \quad . \ (11.)$$

$$\text{Let } D.k' = n; \quad n.\frac{D}{R} = n'; \quad \frac{V^2}{n} = m; \quad \frac{V'^2}{n'} = m' \quad . \ . \ (12.)$$

(1.) For orbit of particle leaving comet at perihelion,

$$e = m + 1;\ p_2 = m . D;\ A = \frac{D}{m+2};\ P = D;\ \Phi = 0;\ \cos\psi = \frac{1}{e} \quad (13.)$$

(2.) For orbit of particle leaving comet at any other point of its orbit,

$$\left.\begin{array}{l} e = \sqrt{m' \sin^2\beta\,(m'+2)+1};\ p_2 = m' \sin^2\beta . R;\ P = \dfrac{p_2}{e-1}; \\ \quad A = \dfrac{R}{m'+2}. \end{array}\right\} \ . \quad (14.)$$

$$\cos\,\Phi = \frac{m' \sin{}^2\beta + 1}{e} \ . \ (15.) \qquad T = c . \text{tang}\left(\text{arc}^\circ\ \frac{\text{tang}\,\Phi}{a}\right) \ (16.)$$

(3.) For position of particle in its orbit,

$$\text{tang}\,\theta = a\left(\text{arc}^\circ\ \text{tang}\,\frac{t}{c}\right) \ . \ (17.) \qquad r = \frac{p_2}{e . \cos\theta - 1} \ . \ (18.)$$

$$\cos\theta = \frac{1}{e}\left(\frac{p_2}{r} + 1\right) \ . \ . \ . \ . \ . \ . \ . \ . \ (19.)$$

$$\Delta = \Theta_1 - \Phi \ . \ . \ . \ (20.) \qquad \gamma = \theta - \Delta \ . \ . \ . \ (21.)$$

If the particle is emitted after the perihelion passage, $\gamma = \theta + \Delta$.

For a particle describing a *hyperbola concave toward the sun*, equs. (13) to (19) become

$$e = m - 1;\ \text{or}\ e = \sqrt{m' \sin{}^2\beta\,(m'-2)+1} \ . \ . \ . \ (22.)$$

$$A = \frac{R}{m'-2};\quad P = \frac{p_2}{e+1};\quad \cos\,\Phi = \frac{m' \sin{}^2\beta - 1}{e} \ . \ . \ (23.)$$

$$r = \frac{p_2}{e . \cos\theta + 1};\quad \cos\theta = \frac{1}{e}\left(\frac{p_2}{r} - 1\right) \ . \ . \ . \ . \ (24.)$$

The true anomaly was calculated from the time, in this case, by an indirect method founded upon the law of areas, which admits of any desired degree of approximation. The calculation might also be made by means of Gauss' formulæ.

In the instance of a repelled particle, which describes an orbit convex toward the sun, the true anomaly was calculated directly by equ. (17). In this equation, and in equation (16) *a* and *c* are constants for the same hyperbolic orbit, but vary from one orbit to another, with the initial circumstances of motion, and the supposed intensity of the repulsive force. The laws of their variation are quite simple, so that their values having been found for one hyperbolic orbit they may be readily computed for any other. These constants having been determined, equ. (17) makes known the true anomaly of a receding particle, for the whole extent of the tail of the comet, with a liability to error not exceeding 2′, and generally much less than this.

The values of r and γ having been found for the position of a particle at any instant of time, we may readily obtain the right ascension and declination of the particle at that instant.

The following equations serve to determine the position of the particle with respect to the axis of the orbit of the comet, and another axis perpendicular to this drawn through the position of the sun.

$$x=r\cos\gamma \quad . \quad . \quad . \quad (25.) \qquad\qquad y=r\sin\gamma \quad . \quad . \quad . \quad (26.)$$

The relative position of any two different particles may be obtained by the equs.

$$\text{tang}\ \varepsilon=\frac{y-y',\ \text{or}\ y'-y}{x-x',\ \text{or}\ x'-x} \ . \ . \ . \ (27.) \qquad d=\frac{x-x',\ \text{or}\ x'-x}{\cos\varepsilon} \ . \ . \ . \ (28.)$$

in which ε = the inclination of the line connecting the two parcles to the axis of the orbit, and d = the distance between the particles.

The same equations will make known the relative position of any particle and the nucleus, at the same instant of time, if x' and y' be taken to represent the coördinates of the nucleus.

The former part of this memoir, published in this Journal, vol. xxix, p. 383, together with the formulas just given, was prepared, and the greater portion of the results of computation now to be published, were obtained more than a year since. The results in question, so far as they relate to the theoretical dimensions of the tail of the comet, were communicated to the American Association for the Advancement of Science, at their last meeting (Aug. 1860). The publication, in this Journal, of the detail of the investigation, has been delayed until leisure could be obtained for a more extended discussion.

The general topic that will first be considered, from our theoretical point of view, is the Dimensions and Form of the Tail of the Comet. A similar numerical investigation will then be made relative to the envelopes of the head. An inquiry will afterwards be instituted into certain special facts and phenomena, with reference to which careful measurements were made by astronomical observers; and an approximate determination given of the period of rotation, and position of the axis of the nucleus. In the light of all the quantitative results obtained, we may perhaps be able to form an adequate conception of the physical processes of development through which the great comet of 1858 passed, as it swept with its "trailing garments" of light through our firmament. We may also obtain some notion of the probable nature of the forces in operation, whose general character is distinctly made out, and whose laws and limits of varying intensity are determined. It will be seen that while from the general conception of the repulsion of the nucleus, combined with that of the sun, each varying in intensity for different cometary particles between certain limits, we may deduce the observed form and dimensions of the train of the comet, as well as of the head, and derive the phenomena of the rise and gradual recess from the nucleus of successive envelopes, we have in a supposed rotation of the nucleus the probable cause of certain special phenomena observed, as the spiral form of the outline of each incipient envelope, the inclination of the first direction of the axis of the tail to the radius-vector produced, &c.

Dimensions and Form of the Train of Donati's Comet,—theoretically investigated, and compared with the results of observation.

We will first observe that if we adopt, for the moment, the prevailing notion that the matter of which the train is made up is directly expelled by the sun from a nebulous envelope surrounding the nucleus, the divergence of the lines of the sun's action tangent to this envelope on opposite sides must have been exceedingly small; since the greatest breadth of the observed envelope was only 40,000 miles, while its distance from the sun was 55,000,000 miles, or more. The breadth of the train, resulting from this divergence at the distance of 55,000,000 miles from the nucleus could not have exceeded 80,000 miles; whereas the actual breadth, at less distances than that, was several millions of miles. If we discard the idea of an envelope in a condition of permanent equilibrium about the nucleus, and take account, in accordance with the Dynamical Theory now under discussion, of the divergent velocities of the jets streaming up from the nucleus, on one side and the other of the radius-vector of the comet, we are still unprovided with a cause adequate to develop a train of the enormous breadth just stated. According to calculations that will be presented in another connection, the velocity in a direction perpendicular to the radius-vector, of any such jet cannot have exceeded $0^{\cdot m}30$ per second and was probably much less. If we suppose this lateral velocity to have obtained, in the instance of the jets inclined under the largest angle to the radius-vector, we find that at the distance of 10,000,000 miles from the nucleus the resulting breadth of the tail, is less than 800,000 miles, while the extreme breadth at that distance, as observed on Oct. 5th, was not less than 3½ millions of miles.

I find, as a result of the detailed discussion I have undertaken, that the great determining cause of the wide lateral dispersion of the matter of the train, consists in an *inequality in the forces exerted by the sun upon different cometary particles.* The other two causes above specified coöperated with this. Upon this hypothetical basis numerous calculations have been made, with the formulæ already given (pp. 54 and 55), and tested by comparison with the results of observation. These calculations consist in determinations of the positions on Oct. $5^{d}\cdot0776$, (W. M. T.) of particles which emerged from the sphere of influence of the nucleus on several previous dates. To these particles were attributed various repulsive and attractive accelerations, due to the sun's action, between certain limits, and also various initial lateral velocities answering to different inclinations to the radius-vector, of the jets proceeding from the nucleus. It will be seen, when we come to investigate the form of envelope that should result from the combined repulsive actions of the nucleus and sun, that if we conceive the ratio of the efficient repulsions of the two bodies to be constant for all points of the nucleus from which the jets proceed, the maximum inclination of a jet to the radius-vector, which answers to the observed form of the cometary envelope, is about 19°; and that if this ratio be supposed to vary from one point of the surface of the nucleus to another, its law of variation, and that of the effective repulsion of the nucleus, must be such that the greatest lateral velocity of a receding particle is nearly the same as if the ratio in question remained constant, and the limiting angle were 19°. The initial lateral velocity of a jet of cometary matter, it is to be observed, increases with the intensity of the effective repulsion of the nucleus. In fact it appears from equation 9^{a}, given in the former part of this memoir, that it varies, for a given angle of emission, a, nearly as the square root of the effective repulsion, p.

The effective repulsive force by which a particle is urged away from the nucleus is the excess of the actual repulsion of the nucleus over its attraction. The same is true of the effective repulsive force of the sun. The actual repulsions must accordingly be distinguished from the efficient repulsive forces by which the particle is solicited. It is assumed, as a fundamental principle, in our investigations, that the *actual* repulsive actions of the sun and nucleus upon any particle vary simply by reason of some change in the condition of the particle; and therefore that the ratio of these actions must remain unalterably the same, through whatever range their actual intensities may be supposed to vary.

The elements of the orbit of Donati's comet employed in the calculations, were computed by Mr. Searle, of the Dudley Observatory, and are as follows:—Per. pass. Sept. $29^{d}\cdot7523$ W. M. T.; $\pi=36°\ 12'\ 21''$; $\Omega=165°\ 18'\ 46''$; $i=116°\ 57'\ 46''$; $\varphi=85°\ 21'\ 21''$; log. $q=9\cdot762236$.

The process of calculation of the position at a given instant, of an individual particle supposed to have left the sphere of influence of the nucleus at a certain previous date, is as follows: Equs. (10) and (11) give the initial circumstances of motion of the particle. The computed value of V' is the initial velocity of the particle, resulting from the motion of the nucleus. This is in a direction parallel to the tangent to the parabolic orbit of the comet, which makes the angle β with the radius-vector. In case the particle is emitted from the nucleus in a direction inclined to the radius-vector it has a certain velocity imparted by the repulsion of the nucleus, relative to the radius-vector. This *lateral velocity*, as it may be termed, is calculated from equs. (7) and (8), and is added to the velocity V' already determined, or subtracted from it, according as the jet proceeds from the preceding or following side of the nucleus. The result is the initial velocity to be attributed to the particle. The particle having the initial, or projectile velocity, thus determined, is urged away from the nucleus into remote space by the repulsive force of the sun, and describes a hyperbolic orbit having the sun in its outer focus. If we supposed it to set out in this orbit at the instant of the perihelion passage of the comet, the sets of equs. (12) and (13) serve for the determination of the orbit; and equs. (17) and (18) make known the true anomaly and radius-vector of the particle in its orbit. If the particle be supposed to leave the nucleus either before or after the perihelion passage, equs. (14) (15) and (16) are employed together with (17 and (18). From equs. (20) and (21) we then obtain the value of γ. The values of r and γ having been determined, we readily calculate the geocentric right ascension and declination of the position of the particle.

The process of calculation for a particle moving under the influence of a diminished gravitation toward the sun, in a concave hyperbolic path, is essentially the same; equs. (22) to (24) now take the place of equs. (13) to (18); and the true anomaly of the particle is determined by an indirect method.

We will take for illustration the calculation of the position on Oct. $5^{d}\cdot0776$, W. M. T., of the particle which left the sphere of influence of the nucleus at the instant of the perihelion passage (Sept. $29^{d}\cdot7523$). We will suppose the particle to have been emitted from the nucleus toward the sun in the line of the radius-vector, and therefore to have had no lateral velocity, or in other words to have had an initial velocity, V, equal to the velocity of the nucleus in its orbit, and in the same direction. We will also take the effective repulsive force of the sun, $k'=1\cdot82$; the attraction of gravitation of the sun, at the same distance, being regarded as unity.

$$D=55{,}000{,}000^{m};\quad V=35^{m}\cdot166;\quad \beta=90^\circ.$$

$k'=\dfrac{1}{48{,}980}$ of a mile; $n=D.k'=1122\cdot907$. (The value of n, as first found, and used in the following calculations, is 1122·95).

$m=\dfrac{V^2}{n}=1\cdot101250$; $e=m+1=2\cdot101250$; $P=55{,}000{,}000^{m}$; $p_2=m.D=60{,}568{,}700$.

$\tan\theta = 0{\cdot}018965 \left(\text{arc}^\circ \tan \frac{t}{19{\cdot}67}\right)$; and t=interval from Sept. $29^d{\cdot}752$ to Oct. $5^d{\cdot}0776 = 5^d{\cdot}325$. The calculation gives $\theta = 16^\circ\ 1'\ 40''$.

$$r = \frac{60{,}568{,}700}{2{\cdot}10125 \cos 16^\circ\ 1'\ 40'' - 1} = 59{,}406{,}100^m. \quad \gamma = \theta = 16^\circ\ 1'\ 40''.$$

With these values of r and γ we obtain,

R. Asc.=214° 1′ 10″; Dec.=22° 23′ 0″ N.

As another example we will determine the position on Oct. $5^d{\cdot}0776$, of a particle which left the sphere of influence of the nucleus on Sept. $24^d{\cdot}360$, and was emitted from the nucleus on its preceding side, and under an angle to the radius-vector of 19°. This is the average limiting angle of emission, for the outer envelope, on Sept. 24th and Oct. 2d. We have $k' = 1{\cdot}213$.

By equs. (10) and (11), $\theta_1 = 16^\circ\ 50'$; $r_1 = 56{,}209{,}000^m$; $V' = 34^m{\cdot}785$; $\beta = 81^\circ\ 35'$. By equs. (7) and (8), (taking $k = 1 + 1{\cdot}213 = 2{\cdot}213$, and reducing from distance $55{,}000{,}000^m$ to $56{,}209{,}000^m$), $v' = 0^m{\cdot}254$. Thus initial velocity$= 34^m{\cdot}785 + 0^m{\cdot}254 = 35^m{\cdot}039$.

$m' = \frac{V'^2}{n'} = 1{\cdot}676085$; $m' \sin^2\beta = 1{\cdot}64009$; $e = \sqrt{m' \sin^2\beta (m' + 2) + 1}$ $= 2{\cdot}65125$. By equ. (15) $\Phi = 5^\circ\ 15'\ 30''$.

Equ. (16) becomes, $T = 24{\cdot}5174 \tan\left(\text{arc}^\circ \frac{\tan\Phi}{0{\cdot}023123}\right)$; whence $T = 1^d{\cdot}7061$.

Interval from Sept. $24^d{\cdot}360$ to Oct. $5^d{\cdot}0776 = 10^d{\cdot}7176$. $t = 10^d{\cdot}7176 - 1^d{\cdot}7061 = 9^d{\cdot}0115$.

Equ. (17) becomes, $\tan\theta = 0{\cdot}023123 \left(\text{arc}^\circ \tan \frac{t}{24{\cdot}5174}\right)$; whence $\theta = 25^\circ\ 0'\ 55''$.

Log $p_2 = \log (m' \sin^2\beta . R) = 7{\cdot}9646743$; and by equ. (18), $r = 65{,}726{,}500^m$.

$\Delta = \Theta_1 - \Phi = 16^\circ\ 50' - 5^\circ\ 15'\ 30'' = 11^\circ\ 34'\ 30''$; $\gamma = \theta - \Delta = 25^\circ\ 0'\ 55'' - 11^\circ\ 34'\ 30'' = 13^\circ\ 26'\ 25''$.

These values of r and γ give

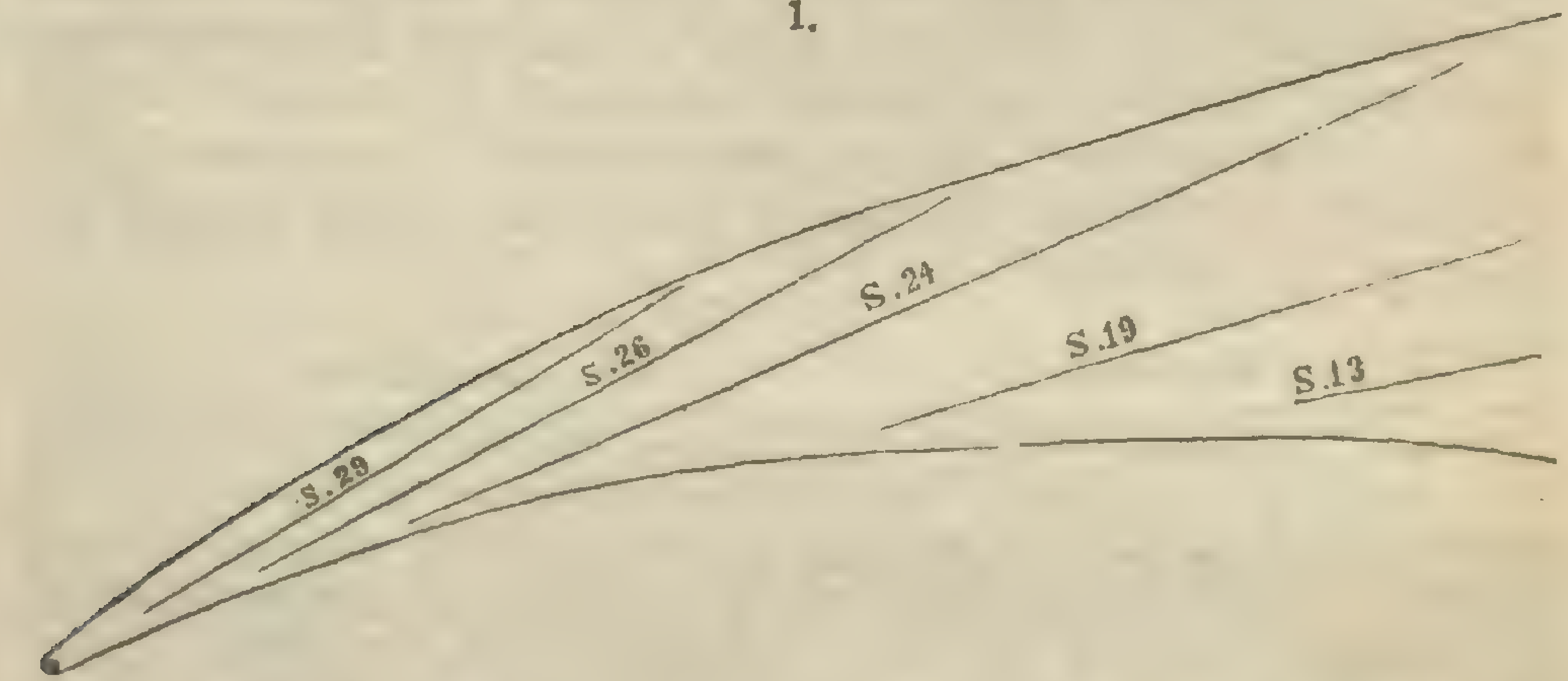
R. Asc.=219° 14′ 40″; Dec.=29° 4′ 20″ N.

RESULTS.

The positions of particles have all been determined for Oct. $5^d{\cdot}0776$. The several dates at which they are supposed to have left the sphere of influence of the nucleus, are Oct. $2^d{\cdot}6043$, Sept. $29^d{\cdot}752$ (Per. pass.), Sept. $26^d{\cdot}9003$, Sept. $24^d{\cdot}360$, Sept. $19^d{\cdot}049$, and Sept. $13^d{\cdot}834$. The several values attributed to the intensity of the sun's effective force are as follows; for the repulsive force, 0, 0·455, 1·213, 1·82, and 2·73; for the attractive force, 0, 0·303, 0·455.

On comparing *the calculated positions of particles which left the region of the nucleus at the same instant, and with the same initial velocity*, but were afterward subjected to different degrees of acceleration, from the action of the sun, it appears that they *are all distributed nearly along a right line, which if indefinitely produced would pass near the nucleus.* This result is illustrated in fig. 1; in which the lines traversing the tail in directions radiating from the nucleus represent the lines of particles that left the nucleus at the several dates above specified. When the calculated positions are plotted upon a large chart, the distribution is found to be very nearly rectilinear. The same fact appears on determining by calculation the relative positions of the several lines connecting the computed places of the particles in the plane of the orbit.

1.



The following table contains the computed coördinates of the positions of particles which left the region of the nucleus with

Part. leaves nucleus.	Force.	η	r	R. Asc.	Dec.
D.		° ′ ″	M.	° ′ ″	° ′ ″
Oct. 2·6043	R, 2·73	16 33 0	57,076,000		
" "	R, 1·213	16 35 6	56,728,500		
Sept. 29·7523	R, 1·82	16 1 40	59,406,000	214 1 10	22 23 0 N.
" "	R, 1·213	16 8 47	58,717,500		
" "	R, 0·455	16 17 50	57,852,600		
" "	R & A=0	16 23 30	57,329,000	212 45 40	20 38 20 N.
" "	A, 0·303	16 27 16	56,943,800	212 31 20	20 18 20 N.
" "	A, 0·455	16 28 30	56,836,800		
Sept. 24·360	R, 1·82	12 28 20	68,066,600	218 25 20	31 35 40 N.
" "	R, 1·213	13 15 0	65,638,700		
" "	R, 0·455	14 13 27	62,453,500		
" "	R & A=0	14 54 50	60,554,000	214 17 20	24 3 40 N.
" "	A, 0·455	15 39 10	58,579,500		
Sept. 19·049	R, 1·82	5 59 36	78,882,000	221 41 55	43 50 0 N.
" "	R & A=0	11 51 0	64,773,000	215 34 20	29 25 50 N.
" "	A, 0·303	13 8 13	62,238,000	214 32 40	26 36 5 N.
" "	A, 0·455	13 53 50	60,974,200		
Sept. 13·834	R & A=0	7 45 50	69,009,000	215 50 40	35 14 10 N.
" "	A, 0·303	10 7 10	65,303,000	214 53 0	30 56 50 N.
" "	A, 0·455	11 25 30	63,396,000	214 21 50	28 37 40 N.
Oct. 5·0776 (nucleus)	....	16 38 0	56,173,500	212 5 30	19 39 20 N.

an initial velocity equal to that of the comet in its orbit. The values of γ and r in those instances in which the right ascension and declination are not given, were obtained by reduction from the determinations made on the supposition of the particles having at the outset certain lateral velocities imparted by the repulsion of the nucleus.

From the values of γ and r contained in the previous table, the results given in the following tables have been obtained. Column 2d gives the inclinations to the axis of the comet's orbit of the lines connecting the several particles with the particle for which the repulsion and attraction are each equal to zero. Column 3d contains the distances, in a direction perpendicular to the same axis, of these lines from the nucleus, expressed in miles. The next column contains the same distances expressed in minutes of arc. The sign +, in column 2d, shows that the line connecting the two particles recedes from the axis of the orbit, as it is produced toward the nucleus. The same sign in column 3d, shows that the line, if produced, would pass in advance of the nucleus.

Oct. 2d·6043.			
Force.	Inclination.	Dist. in miles.	Dist. in min.
	° ′ ″	M.	′
R, 2·73	−10 54 30	+6,232	0·3
R, 1·213	−12 27 0	−8,800	0·4

Sept. 29d·7523.			
Force.	Inclination.	Dist. in miles.	Dist. in min.
	° ′ ″	M.	′
R, 1·82	−6 5 20	−26,500	1·4
R, 1·213	−6 9 30	−28,000	1·5
R, 0·455	−6 0 0	−24,600	1·3
A, 0·303	−7 1 50	−46,000	2·5
A, 0·455	−7 8 0	−48,300	2·6

Sept. 24d·360.			
Force.	Inclination.	Dist.	Dist.
	° ′ ″	M.	′
R, 1 82	+6 21 0	+27,840	1·5
R, 1·213	+5 44 10	−23,000	1·2
R, 0·455	+6 42 0	+49,790	2·7
A, 0·455	+5 58 20	− 4,000	0·2

Sept. 19d·049.			
Force.	Inclination.	Dist.	Dist.
	° ′ ″	M.	′
R, 1·82	+18 35 10	+440,800	24·0
A, 0·303	+16 51 40	+121,000	6 6
A, 0·455	+17 43 40	+281,300	15·3

Sept. 13d·834.			
Force.	Inclination.	Dist.	Dist.
	° ′ ″	M.	′
A, 0·303	+27 44 50	+900,000	49·
A, 0·455	+27 25 10	+762,000	41·

The following table shows the average inclinations of the lines connecting the several particles emitted at each date, with the particle for which the repulsion and attraction are each equal to zero; also the distances of the individual particles from the average lines of direction. When the distance is affected with a negative sign, the particle is found on the side of the line toward the axis of the orbit.

	Average Inc.	Distance.					
		+R, 2·73	R, 1·82	R, 1·213	R, 0·455	A, 0·303	A, 0·455
D.	° ′ ″	′	′	′	′	′	′
Oct. 2·6043	−11 40 45	−0·25		+0·23			
Sept. 29·7523	− 6 29 0		−0·80	−0·43	−0·25	−0·21	−0·33
Sept. 24·360	+ 6 16 12		−0·63	+2 64	−0·84		−0·60
Sept. 19·049	+17 23 30		−18·0			−3·73	+1·42
Sept. 13·834	+27 35 0					+0·73	−1·11

These results show that all the cometary particles which may have been repelled from the nucleus in the line of the radius-vector, at any instant of time, and may have become widely separated by reason of the unequal repulsive actions of the sun upon the different particles, would be found distributed approximately along a right line directed toward the nucleus. If we consider a jet of cometary matter streaming out from the nucleus under an angle to the radius-vector, all of its particles, variously influenced by the sun, are found at the assumed date (Oct. 5^d·0776), distributed nearly along a line diverging somewhat from the line of the particles ejected at the same instant from the nucleus directly toward the sun. It lies in advance of the latter line in the case of a jet emanating from the preceding side of the nucleus, and behind it in the case of a jet proceeding from the following side. The entire collection of matter proceeding at any instant of time, from the various points of the side of the nucleus turned toward the sun, which lie in the plane of the orbit, accordingly forms, as it flows off into space, a band of nebulous matter, the sides of which are somewhat divergent, and the general direction of which is toward the nucleus. The entire train of the comet may be regarded as made up of a series of such bands of cometary matter which left the nucleus at various points of time anterior to the time of observation. Unless the ejection of matter was intermittent, these bands would be infinite in number, and overlap each other. If there were interruptions of continuity in the outflow of matter, they might become separately discernible.

The following are the calculated positions of particles which seem to accord most nearly with the position and form of the convex side of the train, as actually observed:

Left nucleus.		Oct. 2*d*·6043.		Sept. 29*d*·7523.		Sept. 26*d*·9003.		Sept. 24*d*·360.	
Force.	Lat.vel.	R. Asc.	Dec.	R. Asc.	Dec.	R. Asc.	Dec.	R. Asc.	Dec.
	M	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
R, 1·213	+0·255	212 28 20	20 2 50	213 39 50	21 44 0	215 26 40	24 58 20	217 14 40	29 4 20

The lateral velocity here assumed, answers to a jet emanating from the preceding side of the nucleus, under an angle of 19° to the radius-vector, (see p. 58).

The concave, or following side of the tail is best represented by the following calculated positions:

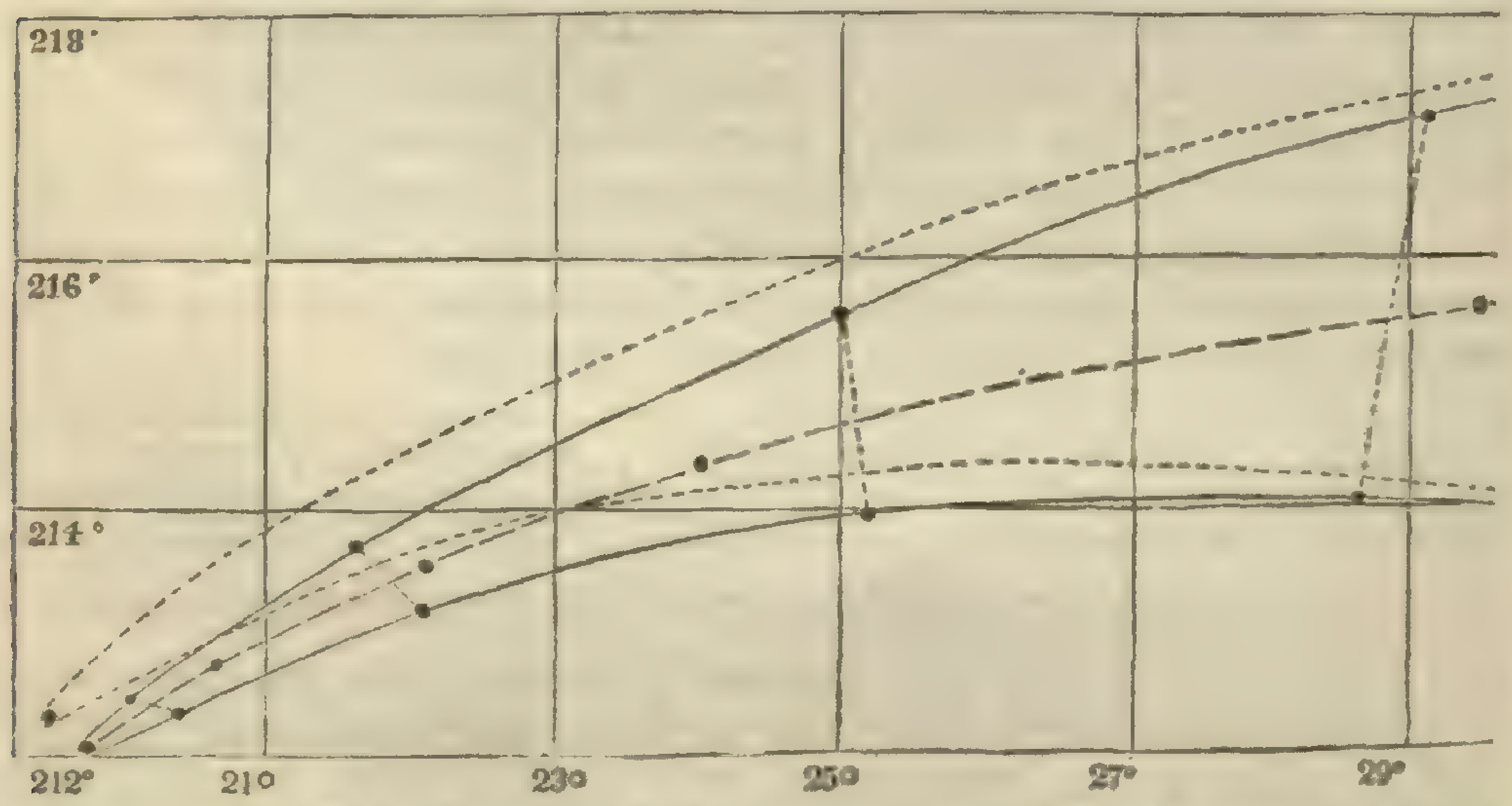
Left nucleus.		Sept. 29*d*·7523.		Sept. 24*d*·360.		Sept. 19*d*·0490.		Sept. 13*d*·834.	
Force.	Lat. vel.	R. Asc.	Dec.	R. Asc.	Dec.	R. Asc.	Dec.	R. Asc.	Dec.
A, 0·455	M −0·184	212° 24′ 50″	20° 16′ 40″	213° 9′ 50″	22° 8′ 0″	213° 52′ 10′	25° 9′ 20″	214° 5′ 20″	28° 37′ 50″

It will be seen, in a subsequent part of the discussion, that the maximum lateral velocity for particles proceeding from the following side of the nucleus, and which may, because of the feebler repulsions they experience, be afterwards found at the concave side of the tail, cannot well exceed $0^{m}·18$; but may be less. In fact it would seem, without going into a detailed calculation, that the concave outline of the tail, for the extent of it under consideration, might be about as well represented by assuming a smaller initial lateral velocity and increasing somewhat the effective attractive force of the sun. · I find, by estimation from the chart, that the limit of the solar attraction answering to a lateral velocity equal to zero, is about 0·61.

In a similar manner we find that the convex outline of the tail may, on the whole, be pretty well represented by discarding the supposed lateral velocity ($0^{m}255$) and augmenting the limit of the solar repulsion. The value of this limit thus obtained is about 1·5. It is important to observe, however, that at moderate distances from the nucleus the breadth of the train was much greater than these limits, with a lateral velocity equal to zero, give; and that the observed breadths at such distances afford indubitable evidence of the existence of considerable lateral velocities imparted, at the outset, by the action of the nucleus.

Fig. 2 will serve to illustrate the comparison of the tail thus theoretically determined with the tail as actually observed at the same date. The figure of the comet, with its train, which is farthest in advance, and whose outline is indicated by a dotted curve, represents the same in its actual position and dimensions,

2.



as carefully determined by Professor Bond, by collating and discussing a large number of European observations. It is reduced from a chart kindly furnished me by Prof. Bond. The full curve lying immediately behind, or below the other, runs through the foregoing calculated positions, and is accordingly the outline of the train, as theoretically determined. The portion of the train represented extends about 10½°, or 10,000,000 miles from the nucleus. The scale of the diagram is too small to admit of anything like an accurate comparison between the two representations. We can only give the results of a comparison made by plotting the two upon a large chart.

The first fact to be observed is that the actual position of the nucleus on Oct. $5^d\cdot0776$ was 18′·6 in advance of the calculated position. (The latter is R. Asc. 212° 5′ 30″, Dec. 19° 39′ 20″ N. Mr. Searle's ephemeris of the comet gives, by interpolation, R. Asc., 212° 3′ 40″, Dec. 19° 39′ 11″ N.) The consequence is that the theoretical falls behind the actual train.

The following table gives the distances of the foregoing calculated positions of particles from the contiguous outline of the tail. The positions are all behind the observed outline.

Particle left nucleus.		Oct. 2*d*·6043.	Sept. 29*d*·752	Sept. 26*d*·900	Sept. 24*d*·360	Sept. 19*d*·049	Sep. 13*d*·834
Force.	Lat. vel.	Dist.	Dist.	Dist.	Dist.	Dist.	Dist.
	M.						
R, 1·213	+0·255	21′·2	25′·4	22′·8	7′·2		
A, 0·455	−0·184		18·6		25·4	18′·6	4′·7

The calculated positions are indicated by dots in the diagram. If they be taken in pairs, on opposite sides of the train, it will be seen that the lines connecting them are approximately perpendicular to its general direction. The lengths of these lines, or the approximate breadths of the train, at the several points, as compared with the lengths of the lines traversing the actual train, in parallel directions and at corresponding distances from the nucleus are given in the following table:

Particles left nucleus.	Oct. 2*d*·6. and Sept. 29*d*·7.	Sept. 29*d*·7 and Sept. 24*d*·3.	Sept. 26*d*·9 and Sept. 19*d*·0	Sept. 24*d*·3. and Sept. 13·*d*8.
Calculated lengths.	14′·5	37′·9	85′·8	168′·6
Actual lengths.	16·5	35·2	86·4	170·7

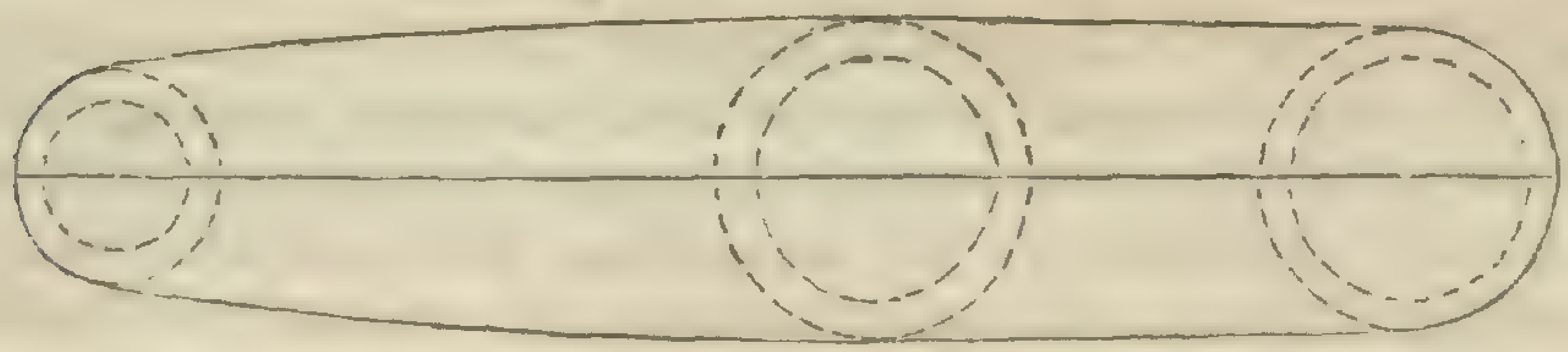
The distances were obtained by measurement from the chart, with a scale of equal parts, and are each approximately equal to the number of minutes in the arc connecting the pair of particles, or in the corresponding arc traversing the observed train.

The broken line lying nearly in the middle of the theoretical train, is traced through the positions occupied by the several particles which left the nucleus at the assumed dates and subsequently continued on in the tangent to the orbit; it being supposed that they were subject to no effective action from the sun, whether repulsive or attractive. It accordingly appears that not far from *one-half of the tail, in breadth, on the concave side was made up of particles that were not effectively repelled by the sun, but separated from the nucleus, after having become disengaged from its influence, only because they gravitated toward the sun with less force than the nucleus did.* It will be seen, in the sequel, that these cometary particles, although subject to an effective attractive action from the sun, were in all probability expelled from the nucleus by a repulsive force, and on leaving its surface had no projectile velocity.

The calculations also show that, except in the immediate vicinity of the nucleus, the particles on the preceding side of the tail left the region of the nucleus several days later than those which are at the same distance from the nucleus on the concave side. For example, at the distance of about 10,000,000 miles we find on the convex side the particle which set out on Sept. $24^{d}·360$, and on the concave side the particle which set out on Sept. $13^{d}·834$.

It will be observed that the calculations hitherto made, have reference to the longitudinal section of the train by the plane of the orbit. All the cometary matter dispersed over this section proceeded originally from points on the exposed surface of the nucleus lying in the plane of the orbit. If we take into consideration the entire collection of matter proceeding from all parts of the surface of the nucleus lying circularly around the point nearest the sun, this, after it has been forced back past the nucleus by the repulsive energy of the sun, will have a conoidal form, with, as will hereafter be seen, a deficiency of matter toward the axis. Regarding it as a hollow conoid its cross section will be a circular ring, or band. On all sides of the conoidal stream will be found particles of matter subject to all the different varieties of solar action, between the two extreme limits, $R=1·213$, and $A=0·455$. The cross section will therefore be composed of as many different circular, or nearly circular rings, as there are different intensities of solar force in operation. These rings at first concentric, and possibly coincident, will separate as they move away from the nucleus, and will at the same time expand by reason of the velocity transverse to the radius-vector originally imparted to their constituent particles by the repulsion of the nucleus. The cross section of the tail of the comet at any supposed distance from the nucleus is accordingly made up of these expanded and separated circular rings; and its outline is the curve enveloping and touching them all. Fig. 3, represents the cross section at the distance of about 10,000,000 miles from the nucleus. Its longest diameter is in the plane of the orbit, and is about 3,600,000 miles. The extreme circular ring on the left, or preceding side, is made up of the particles which are repelled from the sun by the greatest force (1·213), and the extreme ring on the right is composed of particles which have been solicited by the greatest attractive force (0·455). The radius of the former is 236,000 miles, and that of the latter 338,000 miles. The middle circle is composed of particles which are neither attracted nor repelled; its radius is 348,000 miles. The mean breadth of the cross-section of the tail, at the distance supposed, as determined from these three circles, is 615,000 miles; and the ratio of the longest diameter to the mean breadth, is as 5·86 to 1.

3.



We have already intimated (p. 64) that the initial lateral velocity of the particles that go to the extreme concave side of the tail, was probably less than 0^{m}·184, and may have been very small. The mean breadth of the cross section was therefore probably less than 615,000 miles. It may not have exceeded 360,000 miles.

At less distances from the nucleus the cross-section of the train, as determined upon the theory of a repulsion exerted by the nucleus, will deviate less from the circular form. At the very nucleus the cross section perpendicular to the radius-vector, must be almost an exact circle; unless the limiting angle of emission is different on different sides of that line.

These theoretical determinations accord with the results of observations made by Dr. Pape, of Altona, to determine the actual form of the cross-section of the tail at various points. He finds it to have been circular in the vicinity of the nucleus, and at points quite distant from the nucleus to have had a considerably greater extension in the plane of the orbit than in a direction perpendicular to that plane. Dr. Winnecke, of the Pulkova Observatory, in announcing these results deduced from the observations of Dr. Pape, remarks, "We are thus conducted to a remarkable figure of the tail; in the vicinity of the nucleus the transverse sections are circular in form, at greater distances therefrom curves considerably flattened, whose longest diameter probably lies in the plane of the orbit."

In what precedes we have taken account only of the outer envelope of the head of the comet, and of its indefinite prolongation in a continuous stream which we have regarded as the train of the comet. But each of the several envelopes, lying in regular succession within the outer one, is upon the present theory, the round summit of a similar fountain of shining nebulous matter, which flows on continuously past the nucleus into the depths of space. These several conoidal streams lie, at first, one within the other, but at a distance from the nucleus must become more or less intermingled, unless each is composed of particles differently repelled, or attracted by the sun, from those of all the others. The entire luminous stream which we call the tail of the comet is made up of these individual streams equal in number to the number of separate envelopes.

This conception, which is a necessary consequence of the Dynamical Theory under discussion, accords with the notion to which Dr. Winnecke was conducted by his observations, with regard to the actual physical structure of the train of Donati's comet. He intimates that these observations can only be satisfied by the hypothesis of several conoidal trains enclosed one within the other.

It should be observed that in comparing the longitudinal section of the tail of the comet in the plane of the orbit, as theoretically determined, with the tail as actually observed, we have virtually assumed that the plane passing through the line of sight and the general direction of the tail was perpendicular to the plane of the orbit. In point of fact at the assumed date (Oct. $5^{d}\cdot0776$) the inclination of the two planes was about 60°. As a consequence the preceding outline of the visible tail would be a little in advance of the curve of intersection of the plane of the orbit with the preceding surface of the tail; and the following outline a little behind the curve of intersection with the following surface. The tendency of this cause is to diminish slightly the limits of the solar repulsion and attraction.

I propose now to show that *the cometary particles found on the following side of the tail, although subject to an effective, though diminished attraction from the sun, were in all probability expelled from the nucleus by a repulsive action*, after the same manner as those found on the preceding side, and effectively repelled by the sun. In the former portion of this memoir, we obtained the equation $H=\frac{pr}{k}$, or $\frac{H}{r}=\frac{p}{k}$; in which r denotes the radius of the nucleus, H the distance of the vertex of the envelope from the centre of the nucleus, p the *effective* repulsion of the nucleus, or the excess of its actual repulsion over its actual attraction, and k the actual repulsion of the sun. The true value of k is, then, the effective repulsive action of the sun, by which the particle is urged off into space in a hyperbolic orbit, augmented by the sun's attraction of gravitation. In considering the motion of a particle with respect to the nucleus we must regard it as subject to the actual rather than the effective repulsion of the sun, because both the particle and the nucleus must be considered as gravitating toward the sun. The value of k, for the particles of the outer envelope, is then $1{\cdot}213+1=2{\cdot}213$, or $0^{m}{\cdot}0000249$ per second. According to Professor Bond's measurements the greatest value of H may be taken at 14,000 miles, and r as 200 miles (it may possibly be less than this). Thus $p=70k=0^{m}{\cdot}0017430$. If now we regard the density of the solid nucleus as equal to that of the earth, the force of gravity at its surface will be equal to $27{\cdot}37\,A$; A denoting the sun's attraction of gravitation at the distance of the perihelion of the comet ($55{,}000{,}000^{m}$). Let R denote the greatest actual repulsion of the nucleus and a its actual attraction, both as exerted at the surface of the nucleus, then $p=R-a=R-27{\cdot}37\,A$; and, for the particles most energetically repelled, we have

$$\frac{R-a}{2{\cdot}213\,A}=\frac{p}{k}=\frac{H}{r}=70;\ \text{or}\ \frac{R-27{\cdot}37\,A}{2{\cdot}213\,A}=70.\quad \text{Thus}\ R=182{\cdot}28\,A.$$

Now, for the particles subject to the least actual repulsion from the sun, and to an effective attraction, $0{\cdot}455\,A$, as they move off in their hyperbolic orbits, and which as we have seen go to make up the following side of the tail, we must take $k=A-0{\cdot}455\,A=0{\cdot}545\,A$; and $\dfrac{2{\cdot}213\,A}{0{\cdot}545\,A}=4{\cdot}060$. For these particles we have $p'=R'-a$; and since we must suppose that the actual repulsive action of the nucleus will increase or decrease with the changing physical condition of the particle, in the same ratio as that of the sun, we shall have $R'=\dfrac{R}{4{\cdot}060}=\dfrac{182{\cdot}28\,A}{4{\cdot}060}=44{\cdot}9\,A$. Whence $p'=R'-a=44{\cdot}90\,A-27{\cdot}37\,A=17{\cdot}53\,A$. It thus appears that if we assume its density to be the same as that of the earth, the nucleus would still exert an effective repulsion upon the particles which gravitate toward the sun with the force $0{\cdot}455\,A$, and are found distributed along the following side of the train. If we suppose the density of the nucleus to be 1·75 instead of 1, we obtain for the value of p', $2{\cdot}05\,A$. But if it be assumed equal to twice the density of the earth, we get $p'=-3{\cdot}10\,A$. Thus upon this supposition the effective action of the nucleus becomes an attractive force, and hence the particles under consideration could not leave the nucleus.

It is to be observed that if p' comes out a feeble repulsive force, as in the case in which the density is taken equal to 1·75, the initial lateral velocity of the particles subject to its action must be much less than the velocity ($0^{m}{\cdot}184$) employed in our calculations for the concave side of the tail, and that if a smaller lateral velocity be adopted the observed form and position of the concave side cannot be represented without supposing the effective attraction of the sun to be greater than 0·455. But if this attraction be only increased to 0·500, we get $p' = -2{\cdot}1\,A$, and the particles therefore could not leave the nucleus. We may conclude therefore that the *mean density of the nucleus is in all probability less than* 1·75; the mean density of the earth being taken equal to unity.

If we attribute to the density of the nucleus decreasing values, less than unity, we obtain larger and larger positive values for p'; which however cannot possibly exceed $38\,A$. Upon every such supposition, therefore, there would be an effective repulsive force to expel the particles in question from the nucleus. But the idea here naturally suggests itself that the *limit to the solar attraction, for the concave side of the tail, may have been precisely that value which did not admit of any increase without converting the effective action of the nucleus into an attractive force.* If this plausible idea be admitted, we have for the limit in question the value answering to a lateral velocity equal to zero, which, as we have seen, is 0·61.

From this result we deduce for the probable density of the nucleus, 1·25. If the density be supposed greater than this, p' becomes an attractive force, and if it be taken materially less, p' is no longer the feeble repulsive force which the hypothesis of a limit resulting simply from the impossibility of the particles being repelled from the nucleus requires.*

From the calculations which have just been made we may gain some insight into the probable nature of the forces of repulsion exerted by the sun and nucleus upon cometary particles. We find that *the ratio of the actual repulsive forces exerted by the two bodies is not the same as that of their actual attractive forces.* Thus upon the supposition that the density of the nucleus is equal to that of the earth, the ratio of the attractive forces at the surface of the nucleus, is 27·37, while the ratio of the repulsive forces is equal to $\frac{182{\cdot}28\,A}{2{\cdot}213\,A}$, or 82·4. We obtain similar results, if we attribute other values to the density, as will be seen from the comparison made in the following table.

Density.	Ratio of Attr.	Ratio of Rep.	Density.	Ratio of Attr.	Ratio of Rep.
1	27	82	0·75	20	79
1·75	48	92	0·50	14	76
2	55	95	0·25	7	73
3	82	107	0·10	2·7	71

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* In effecting this determination of the density it is tacitly supposed that the effective repulsion of the nucleus becomes changed into an attraction for the particles that would be ejected in the direct line toward the sun. If, as we shall hereafter see there is reason to believe, this change should occur only for the particles emitted under an angle to the radius-vector, and because of a diminution in the actual repulsions of the nucleus and sun as this angle becomes greater, the above determination of density (1·25) must be discarded.

The two ratios approach equality as the assumed density is greater, but they cannot become equal however great the density may be supposed to be. On the other hand however small the density may be taken, and consequently also the ratio of the attractive forces, the ratio of the actual repulsions cannot be less than 70. Now the ratio of the actual attractive forces exerted by the two bodies is the same as that of their masses, the ratio of the repulsions is then greater than that of the masses. We may hence conclude that *the repulsion exerted by the sun, and also by the nucleus, is not a property belonging to all the particles of the mass*, like the attraction of gravitation. It is probable therefore, that it is either a magnetic, or an electric force, emanating from the surface of the body, or from a portion only of its mass.

Yale College, May 10th, 1861.

(*To be continued.*)

ART. XII.—*The Great Auroral Exhibition of Aug. 28th to Sept. 4th*, 1859.—7TH ARTICLE; by ELIAS LOOMIS, Professor of Natural Philosophy and Astronomy in Yale College.

SINCE the publication of my last auroral article I have obtained some additional information, chiefly collected during a recent visit to Europe.

1. *Observations at Highland, Illinois*, (lat. 38° 43′, long. 89° 48′ W.), *by* A. F. BANDELIER, Jr.

At 9 P. M. Aug. 28, 1859, I was struck by the appearance of a broad purple ray extending lengthwise across the seven stars of Ursa Major to 80° of height. This ray remained for about half an hour, rapidly changing. Then appeared three rays in the east inclining to the south, which ascended from a bright yellow circle resting upon a segment of a brown misty appearance. Both arch and segment were gradually rising, the former illuminated as by the faint lightning of a distant tempest. The segment greatly agitated near its upper border, tossing and rolling its cloudy particles over each other in heavy undulations. No more rays appeared, but the yellow arch and the segment rose slowly. Through the latter I saw plainly α Aurigae rise without much diminution of brightness.

At 1 o'clock a quantity of rays shot upwards from the lucid arch, purple at the base and middle, brilliant yellow at the top. A little S.E. from the zenith they united, forming a small semicircle of the most dazzling beauty, from which rays now shot downwards. The corona lasted only a few minutes, then broke up and vanished. Some rays continued after it, but the great movement of the arch and segment ceased gradually.

At 4 A. M. both still stood on the northern horizon. The greatest height of the arch during the whole apparition was 60°, that of the segment was 20° to 25°.

Sept. 2d at 9 P. M. I observed a dark segment in the north looking very much like a fog, of an irregular circular form, the upper borders broken up, and 5° or 6° above the horizon at its greatest elevation. Behind it a faint light broke out, not unlike a distant prairie fire. The whole ranged from 10° west to 40° east. At 9^h 20^m four rays darted out directly north. They were of a pale milky color. They seemed to descend into the segment below, and then suddenly prolonged themselves into the true ray or flame. The same lightning-like illumination of the arch was visible as in the aurora of Aug. 28th.

At 9^h 30^m a strong ray appeared N. 20° E.

At 9^h 35^m light diminishing east.

At 9^h 45^m strong decrease. Segment almost without motion; its borders were now completely regular; continued to decrease and fall below the horizon.

At 10^h 30^m only feebly visible.

At 11^h 2^m segment only a small stripe of 2° in breadth, a faint lighted border.

At 3^h 30^m A. M. all had entirely vanished. Clouds were gathering from the south.

2. *Observations at Greenwich, England*, (lat. 51° 28′), *communicated by* Prof. G. B. AIRY, Astronomer Royal of Great Britain.

Greenwich mean time.	Western declination.	Greenwich mean time.	Horizontal force.	Greenwich mean time.	Vertical force.
h m	° ′ ″	*h m*		*h m*	
August 28th.		August 28th.		August 28th.	
0 0	21 31 30	0 0	·0897	0 0	·02143
5 23	21 12 30	2 15	·0953	3 15	·02170
8 15	21 17 15	3 30	·0890	6 40	·01969
11 15	21 52 10	5 0	·0926	9 4	·01880
11 20	20 57 30	5 25	·0935	10 39	·02019
11 38	21 52 10	7 45	·0904	11 46	·01510
12 46	20 47 10	8 19	·0897	12 37	·00260
14 4	21 53 5	9 15	·0916	13 2	·01561
14 45	21 16 50	9 51	·0929	13 21	·00263
16 5	22 1 50	21 0	·0832	14 9	·01836
17 11	21 3 0			14 27	·01340
18 9	21 23 30			16 49	·02263
18 55	21 5 30			18 10	·00840
19 6	21 55 40			22 6	·02251
21 15	21 4 10			22 43	·02120
August 29th.		August 29th.		August 29th.	
0 0	21 34 10	0 0	·0845	0 0	·02143
2 39	21 27 10	3 13	·0933	2 30	·01989
5 28	21 7 10	3 50	·0958	5 10	·01556
5 50	21 24 20	4 9	·0809	5 26	·01580
7 20	21 16 30	5 30	·0895	7 22	·01352
14 26	28 0	18 27	·0865	12 28	·01464
19 46	12 0	19 39	·0882	14 58	·01654
23 45	29 30	21 56	·0844	19 20	·02229
August 30th.		August 30th.		August 30th.	
0 0	21 28 20	0 0	·0861	0 0	·01882
5 20	20 25	2 39	·0886	9 18	·01349
12 52	27 50	7 45	·0880	18 47	·01963
19 3	15 0	14 14	·0893	23 15	·01730
21 37	20 30	21 45	·0854	23 59	·01481

Table continued.

Greenwich mean time.	Western declination.	Greenwich mean time.	Horizontal force.	Greenwich mean time.	Vertical force.
h m	° ′ ″	h m		h m	
August 31st.		August 31st.		August 31st.	
0 0	21 46 10	0 0	·0867	0 0	·01481
7 6	18 40	7 18	·0900	5 15	·00950
9 22	9 30	9 20	·0909	16 40	·01563
14 10	23 40	12 45	·0883	17 30	·00301
16 17	22 5 20	15 53	·0930	17 45	·04158
19 45	20 57 45	18 47	·0839	17 56	·02297
20 12	20 31 10	19 30	·1027	18 20	·04261
20 14	21 7 10	20 10	·0763	18 30	·02722
20 16	20 55 10	20 15	·0940	18 41	·04139
20 18	21 22 20	20 41	·0841	19 6	·02228
20 24	20 56 25	20 50	·0938	19 15	·02817
20 27	21 24 10	21 19	·0787	20 15	·02031
21 40	21 1 30	21 44	·0947	20 33	·02166
22 10	21 26 10	22 15	·0777	21 37	·02097
22 42	21 10 0	23 10	·0888	23 20	·01730
September 1st.		September 1st.		September 1st.	
0 0	21 33 35	0 0	·0885	0 0	·01719
1 56	42 35	1 57	·0906	8 33	·01051
8 20	15 0	4 28	·0885	21 0	·01658
14 10	25 35	9 21	·0900	23 30	·01563
20 0	12 30	13 0	·0905	23 43	·01438
23 42	35 40	20 55	·0873	23 56	·02043
23 54	8 45	22 40	·0869		
September 2d.		September 2d.		September 2d.	
0 0	21 18 0	0 0	·0940	0 0	·01978
0 16	4 10	1 33	·1069	1 22	·02556
1 4	53 10	1 39	·0817	1 36	·01963
1 13	11 5	1 59	·1065	1 53	·02437
1 18	52 15	2 27	·0778	2 17	·02197
1 40	21 10	2 52	·1120	2 48	·02769
1 54	58 50	3 23	·1008	3 13	·02208
2 13	11 15	3 37	·1078	3 30	·02400
2 30	51 40	5 43	·0930	5 47	·01988
2 43	13 10	6 30	·0964	6 3	·02142
3 40	51 25	8 40	·0850	8 37	·01602
3 57	23 10	9 25	·0937	9 5	·01670
5 50	40 0	11 29	·0802	10 47	·01200
6 15	0 30	12 43	·0945	11 17	·01329
7 0	24 10	12 54	·0816	11 32	·01063
8 51	20 53 0	14 48	·0884	12 37	·01293
9 40	21 32 0	16 13	·0816	12 56	·01142
11 4	21 6 25	18 25	·0848	14 40	·01290
11 43	21 42 20	21 35	·0812	14 57	·01221
14 15	21 5 30	23 12	·0833	15 50	·01320
15 52	27 20			16 6	·01281
20 15	15 30			18 48	·01556
September 3d.		September 3d.		September 3d.	
0 12	21 27 20	0 0	·0889	0 0	·01739
3 3	58 45	0 21	·0940	3 30	·01620
5 53	21 10	3 15	·0837	4 40	·01946
6 12	20 50 5	5 9	·1075	5 32	·01683
6 53	21 28 15	6 27	·0887	6 4	·01936
7 30	0 30	6 31	·0930	6 58	·01300

Table continued.

Greenwich mean time.	Western declination.	Greenwich mean time.	Horizontal force.	Greenwich mean time.	Vertical force.
h m	° ′ ″	*h m*		*h m*	
September 3d.		September 3d.		September 3d.	
7 58	21 28 30	8 5	·0835	7 26	·01405
8 15	11 30	10 44	·0891	8 4	·01347
11 50	36 0	12 39	·0811	8 15	·01425
13 8	15 10	13 45	·0850	12 36	·00900
16 7	25 0	15 15	·0822	15 7	·01510
21 6	13 0	19 21	·0861	19 38	·01923
September 4th.		September 4th.		September 4th.	
0 0	21 33 0	0 0	·0830	0 0	·01809
1 20	41 45	2 15	·0936	2 4	·01763
2 37	22 30	3 22	·0845	2 36	·01846
8 30	30 20	3 52	·0912	3 37	·01682
10 54	12 10	5 19	·0837	3 56	·01701
12 4	30 30	12 55	·0877	14 4	·01102
19 28	3 30	13 54	·0827	21 58	·01732
23 15	37 10	19 32	·0883	23 59	·01739
		23 12	·0790		

3. *Deflections of the needles of the Vertical Galvanometers of Cooke and Wheatstone's Telegraph instruments, observed at Ramsgate Station in the County of Kent, England, upon three distinct lines of telegraph: namely, Ashford and Margate, distant in a direct line 27½ miles; Ashford and Ramsgate, distant 27½ miles; Ramsgate and Margate, 3 miles;* furnished by Mr. CHARLES V. WALKER.

Note.—The direction in which the current moves is indicated by the letters N and S; N means that the current is from the more northerly to the more southerly station of the two; S means the reverse. The direction of Ashford from Ramsgate is S. 60° W., and that of Margate from Ramsgate N. 22° W.

In the "value column," "strong" means 30° or 40°; "hard over," 45°; horizontal from 70° to 80°. Ordinary strong telegraph signals produce about 60°.

Date.	Time.		Telegraph Line.	Direction.	Value.
1859. Aug. 29th	7·10 A. M.	7·25 A. M.	Ashford and Margate,	S	Strong.
"	7·36 "	7·45 "	" " "	S	Hard over.
"	7·46 "	7·49 "	" " Ramsgate,	N	Strong.
"	7·50 "	8· 0 "	" " "	S	Hard over.
"	9·45 "	10· 0 "	" " "	N	" "
"	10·20 "	10·27 "	" " "	N	Strong.
"	10·27 "	10·28 "	" " "	S	Hard over.
"	10·28 "	10·36 "	" " "	N	Strong.
"	10·37 "	10·40 "	" " "	S	Hard over.
"	10·40 "	10·45 "	" " Margate,	N	" "
"	10·45 "	10·49 "	Margate and Ashford,	S	" "
"	10·50 "	10·53 "	" " "	N	" "
"	10·53 "	11· 0 "	" " "	S	Horizontal.
"	11· 2 "	11·25 "	" " "	N	"
"	11·25 "	11·40 "	" " "	N	Hard over.
"	11·45 "	12·20 P. M.	" " "	N	" "
"	12·30 P. M.	12·45 "	" " "	N	Strong.
"	12·48 "	1· 3 "	" " "	N	"
"	1· 5 "	1·40 "	" " "	S	"

Table continued.

Date.	Time.		Telegraph Line.	Direction.	Value.
1859. Aug. 29th	2·40 P. M.	2·53 P. M.	Margate and Ashford,	N	Very Strong.
“	3·40 “	3·50 “	“ “ “	N	“ “
“	3·52 “	4· 5 “	“ “ “	S	Horizontal.
“	3·52 “	4· 5 “	Ashford and Ramsgate,	S	Very strong.
“	4·15 “	4·50 “	“ “ Margate,	N	“ “
“	5· 0 “	5·20 “	“ “ “	N	“ “
“	“ “	“ “	“ “ Ramsgate,	N	“ “
“	5·25 “	5·48 “	“ “ Margate,	N	“ “
“	6·10 “	6·23 “	“ “ “	S	“ “
“	6·50 “	7·20 “	“ “ “	S	“ “
“	7·53 “	8·10 “	“ “ “	S	Slight.
Sept. 1st	11·20 A. M.	11·26 A. M.	“ “ “	S	Strong.
“	11·28 “	11·35 “	“ “ “	N	Slight.
Sept. 2d	7·10 “	7·42 “	“ “ “	N	Horizontal.
“	7·10 “	7·50 “	Ramsgate and Margate,	N	“
“	7·10 “	7·42 “	Ashford and Ramsgate,	N	“
“	7·43 “	7·48 “	“ “ Margate,	S	Strong.
“	“ “	“ “	“ “ Ramsgate,	S	“
“	7·49 “	7·51 “	“ “ Margate,	N	Hard over.
“	“ “	“ “	“ “ Ramsgate,	N	“ “
“	7·51 “	7·56 “	“ “ Margate,	S	“ “
“	“ “	“ “	“ “ Ramsgate,	S	“ “
“	7·56 “	8· 0 “	“ “ Margate,	N	“ “
“	“ “	“ “	“ “ Ramsgate,	N	“ “
“	“ “	“ “	Ramsgate and Margate,	N	“ “
“	8· 0 “	8· 7 “	Ashford and “	S	Strong.
“	“ “	“ “	“ “ Ramsgate,	S	“
“	“ “	“ “	Ramsgate and Margate,	S	“
“	8· 8 “	8·12 “	Ashford and “	S	“
“	“ “	“ “	“ “ Ramsgate,	S	“
“	“ “	8·17 “	Ramsgate and Margate,	S	“
“	8·12 “	“ “	Ashford and Ramsgate,	S	Hard over.
“	“ “	“ “	“ “ Margate,	S	“ “
“	8·20 “	8·30 “	“ “ Ramsgate,	N	“ “
“	“ “	“ “	“ “ Margate,	N	“ “
“	8·31 “	8·46 “	“ “ “	S	“ “
“	“ “	8·40 “	“ “ Ramsgate,	S	“ “
“	8·41 “	8·46 “	“ “ “	N	“ “
“	“ “	“ “	“ “ Margate,	N	“ “
“	8·47 “	8·54 “	“ “ “	S	“ “
“	“ “	“ “	“ “ Ramsgate,	S	“ “
“	8·54 “	9· 0 “	“ “ “	N	Strong.
“	“ “	“ “	“ “ Margate,	N	“
“	9·22 “	9·25 “	“ “ “	N	“
“	9·26 “	9·28 “	“ “ “	S	“
“	9·29 “	9·40 “	“ “ “	N	“
“	9·40 “	9·52 “	“ “ “	S	“
“	9·55 “	10·32 “	“ “ “	N	“
“	“ “	“ “	“ “ Ramsgate,	N	“
“	10·35 “	10·38 “	“ “ Margate,	S	“
“	10·38 “	10·40 “	“ “ “	N	“
“	10·41 “	10·46 “	“ “ “	S	“
“	10·55 “	11· 0 “	“ “ “	S	“
“	11· 2 “	11·15 “	“ “ “	N	“
“	“ “	“ “	“ “ Ramsgate,	N	“
“	11·16 “	11·27 “	“ “ Margate,	S	“
“	11·20 “	11·32 “	“ “ Ramsgate,	S	“
“	11·38 “	11·40 “	“ “ Margate,	N	“
“	11·40 “	11·45 “	“ “ “	S	“
“	11·45 “	11·49 “	“ “ “	N	“
“	“ “	“ “	“ “ Ramsgate,	N	“
“	“ “	11·50 “	Ramsgate and Margate,	N	“
“	11·50 “	11·51 “	Ashford and “	S	“
“	“ “	“ “	“ “ Ramsgate,	S	“
“	“ “	“ “	Ramsgate and Margate,	S	“
“	11·52 “	11·54 “	Ashford and Ramsgate,	N	

Table continued.

Date.	Time.		Telegraph line.	Direction.	Value.
1859. Sept. 2d	11·52 A. M.	11·54 A. M.	Ramsgate and Margate,	N	Strong.
"	" "	" "	Ashford and "	N	"
"	11·59 "	12· 3 P. M.	" " Ramsgate,	N	"
"	" "	" "	" " Margate,	N	"
"	" "	" "	Ramsgate and "	N	"
"	12· 4 P. M.	12·14 "	Ashford and Ramsgate,	S	Horizontal.
"	" "	" "	" " Margate,	S	Strong.
"	12·15 "	12·30 "	" " "	N	Horizontal.
"	" "	" "	" " Ramsgate,	N	Strong.
"	12·30 "	12·35 "	" " Margate,	N	"
"	" "	" "	" " Ramsgate,	N	"
"	" "	" "	Ramsgate and Margate,	N	"
"	12·36 "	12·57 "	Ashford and "	N	"
"	" "	" "	" " Ramsgate,	N	"
"	12·57 "	1·18 "	" " Margate,	S	"
"	" "	" "	" " Ramsgate,	S	"
"	" "	" "	Ramsgate and Margate,	S	"
"	1·20 "	1·44 "	Ashford and "	N	"
"	" "	" "	" " Ramsgate,	N	"
"	" "	" "	Ramsgate and Margate,	N	"
"	1·44 "	1·47 "	Ashford and Ramsgate,	S	"
"	" "	" "	" " Margate,	S	"
"	1·47 "	1·54 "	" " "	N	"
"	" "	" "	" " Ramsgate,	N	"
"	2· 0 "	2·15 "	" " Margate,	N	"
"	" "	" "	" " Ramsgate,	N	"
"	2·15 "	2·18 "	" " Margate,	S	"
"	" "	" "	" " Ramsgate,	S	"
"	2·21 "	2·31 "	" " Margate,	S	Horizontal.
"	" "	2·37 "	" " Ramsgate,	S	"
"	2·38 "	2·52 "	Ramsgate and Margate,	N	Strong.
"	" "	" "	Ashford and Ramsgate,	N	"
"	" "	" "	" " Margate,	N	"
"	2·52 "	2·55 "	Ramsgate and "	S	"
"	" "	" "	Ashford and Ramsgate,	S	"
"	" "	" "	" " Margate,	S	"
"	2·55 "	3· 2 "	Ramsgate and "	N	"
"	" "	" "	Ashford and Ramsgate,	N	"
"	" "	" "	" " Margate,	N	"

4. *Auroral observations made at Sea; furnished by* Rear-Admiral ROBERT FITZ ROY, of the British Navy.

A. Lat. 50° 47′ N., long. 10° 12′ W.

Aug 28th. About 11^h 30^m P. M. the sky being cloudy, it brightened up like daybreak, remained so for twenty minutes, then turned a dark red, and soon after darkened in again as before.

B. Lat. 29° 48′ N., long. 45° 20′ W.

Aug. 28th. The aurora seen from 9 P. M. till 4 A. M. the next morning, of a rose color. Streamers about 30° high.

C. Lat. 26° 48′ N., long. 45° 40′ W.

Aug. 28th. Sky in the S.S.E. of a lurid fiery color; a vivid bright streak from the middle.

D. Lat. 25° 45′ N., long. 27° 4′ W.

Aug. 28th. From 11^h 15^m P. M. till midnight the N.W. portion of the sky of a deep red color, resembling an angry sunrise.

E. Lat. 33° 55′ N., long. 44° 13′ W.

Sept. 2d. The aurora faintly visible in the north about 4 A. M.

F. Lat. 33° 33′ N., long. 33° 2′ W.

Sept. 2d. At 3 A. M. a low bank of straw colored aurora on the northern horizon; it became a beautiful rose color, covering about four-tenths of the sky and gradually disappeared as the day broke.

G. Lat. 24° 10′ N., long. 35° 50′ W.

Sept. 2d. Aurora seen in the morning from N.W. to E.N.E. of a bright red color, interspersed with streaks of white, converging to a center nearly over the ship.

5. *State of the weather at the Russian magnetic observatories, during the Auroral display of Aug. 28th to Sept. 2d,* 1859; *furnished by* A. T. KUPFFER, Director of the Central Physical Observatory.

Hour.	St. Petersburg.	Catherinburg.	Barnaoul.	Nertchinsk.
	August 28th, 1859.			
0	overcast	cloudy	scat. clouds	overcast
1	overcast	overcast	scat. clouds	overcast
2	cloudy	overcast	scat. clouds	overcast
3	cloudy	overcast	scat. clouds	overcast
4	cloudy	cloudy	scat. clouds	overcast
5	cloudy	cloudy	clouds in hor'n	overcast
6	cloudy	cloudy	clouds in hor'n	overcast
7	cloudy	cloudy	clouds in hor'n	clouds in hor'n
8	cloudy	cloudy	clear	clouds in hor'n
9	cloudy	cloudy	clear	cloudy
10	light clouds	cloudy	clear	cloudy
11	light clouds	clouds in hor'n	clear	cloudy
12	light clouds	clear	clear	cloudy
13	cloudy	clear	clear	overcast
14	cloudy	clear	clear	overcast
15	cloudy	clear	clear	overcast
16	cloudy	clear	clouds in hor'n	overcast
17	cloudy	clouds in hor'n	clouds in hor'n	overcast
18	cloudy	clouds in hor'n	clouds in hor'n	overcast
19	cloudy	clouds in hor'n	clouds in hor'n	overcast
20	cloudy	clouds in hor'n	clouds in hor'n	clouds in hor'n
21	cloudy	cloudy	scat. clouds	clouds in hor'n
22	cloudy	cloudy	scat. clouds	cloudy
23	cloudy	cloudy	scat. clouds	cloudy
	September 2d, 1859.			
0	scat. clouds	cloudy	cloudy	cloudy
1	scat. clouds	cloudy	scat. clouds	cloudy
2	scat. clouds	cloudy	cloudy	cloudy
3	light clouds	cloudy	cloudy	overcast
4	light clouds	cloudy	cloudy	cloudy
5	light clouds	cloudy	cloudy	overcast
6	light clouds	cloudy	cloudy	overcast
7	light clouds	clouds in hor'n	cloudy	cloudy
8	scat. clouds	clouds in hor'n	cloudy	clouds in hor'n
9	scat. clouds	clouds in hor'n	cloudy	clear
10	light clouds	cloudy	cloudy	clear
11	scat. clouds	cloudy	cloudy	clouds in hor'n
12	scat. clouds	clouds in hor'n	scat. clouds	clouds in hor'n
13	scat. clouds	clouds in hor'n	scat. clouds	clouds in hor'n
14	scat. clouds	clouds in hor'n	clouds in hor'n	clouds in hor'n
15	scat. clouds	clouds in hor'n	clouds in hor'n	clouds in hor'n
16	scat. clouds	clouds in hor'n	clouds in hor'n	clouds in hor'n
17	light clouds	clouds in hor'n	clouds in hor'n	cloudy
18	light clouds	clouds in hor'n	scat. clouds	cloudy
19	light clouds	clouds in hor'n	scat. clouds	cloudy
20	light clouds	clouds in hor'n	scat. clouds	overcast
21	light clouds	scat. clouds	scat. clouds	overcast
22	light clouds	cloudy	scat. clouds	cloudy
23	light clouds	cloudy	scat. clouds	overcast

In vol. xxx, pp. 80–82, of this Journal, observations are published showing an unusual disturbance of the magnetic instruments throughout the whole of the Russian empire, but no mention is made of any aurora. The preceding observations show that during this period the sky was generally overcast at each of the Russian stations.

6. *Observations of the Aurora of Aug.* 28*th and* 29*th*, 1859, *made in Australia ; furnished by* Mr. JAMES GLAISHER, of the Greenwich Observatory.

A. Observations at Hobarton, lat. 42° 52′ S., long. 147° 27′ E.

Aug. 29th, from 6^h 55^m to 7^h 25^m P. M. there appeared a most brilliant aurora extending from W. by N. to the eastern part of the horizon in one continuous arc of about 190°, and shooting up to the zenith. The eastern and western extremities of the conoid were of a pale ruby and deep red color, intermixed through the whole vault with bands of pale yellow and shades of dark and light green, and with here and there a small dark cloud jutting in; elsewhere the circumpolar stars glittered like diamonds set in an emerald and ruby ground. The phenomenon had for 30 minutes a most magnificent appearance, the bands being in complete repose, forming a truncated cone of glory, the apex of which, if projected, would have terminated in the zenith. This brilliant storm appeared again about 9^h 30^m P. M., flickering in brisk coruscations of most beautiful color from the horizon to the zenith.

A second display of the aurora appeared on the night of Sept. 2d, equally brilliant and extensive and less transitory. From midnight to 1 A. M. the aurora broke out into flickering streamers and coruscations, forming in the zenith a well defined corona, which shortly after became diffused and then dispersed.

B. Observations at Cape Otway, lat. 30° 51′ S., long 143° 50′ E.

Aug. 29th. Aurora most magnificent at 6^h 30^m P. M. and continued visible until after 2 A. M., displaying itself in the form of a rainbow, the arc extending to about 60° or 70°. First color above the horizon a light blue with a tint of green, blending into second, a very light yellow, again blending into third, a deep red.

C. Observations at Portland, lat. 38° 20′ S., long. 141° 55′ E.

Aug. 29th. Aurora visible at 6^h 40^m P. M. At 7 P. M. a bright band partly tinged with blue and pink, extending E. and W., pink rays converging to a centre on the band a little to the W. of the Milky Way. Gradually faded, and all disappeared by 8 P. M.

D. Observations at Melbourne Observatory, lat. 37° 49′ S., long 145° 9′ E., by GEORGE NEUMEYER.

On the evening of Aug. 28th great disturbances made themselves manifest in all the three magnetic elements, which became less violent during the early part of the morning of the 29th. At 4 A. M. Aug. 29th, the horizontal intensity was 0·0020 below the mean for the previous ten days, and then increased until 8^h 50^m A. M. when the disturbances assumed so violent a character that the intensity at times, and the inclination very frequently, could not be registered, the scales being out of the field of the telescope. At 8^h 57^m A. M. the horizontal intensity was 0·0284 below the mean above referred to, showing a decrease of 0·0264 in the space of one hour. The variation of the needle underwent similar changes, decreasing rapidly until 9^h 35^m, when it was 36 minutes below the mean for the ten days mentioned above. After 8 A. M. the magnetic instruments were registered every minute. The following table contains the means for declination and horizontal intensity.

	Declination.	Horizontal intensity.
Between 7^h and 8^h A. M.	8° 24′·20	2·36264
“ 8 “ 9 “	8 22·23	2·33677
“ 9 “ 10 “	8 8·52	2·35072
“ 10 “ 11 “	8 13·00	2·34711
“ 11 “ 12 “	8 22·86	2·34983
“ 12 “ 1 P. M.	8 34·33	2·35160
“ 1 “ 2 “	8 37·54	2·35539
“ 2 “ 3 “	8 37·40	2·35755
“ 3 “ 4 “	8 38·46	2·34353
“ 4 “ 5 “	8 35·83	2·35479
“ 5 “ 6 “	8 34·21	2·35412

The above figures do not give the greatest range; that for declination being 1° 8′·8; and for intensity 0·03197 of the absolute unit.

At 6^h 10^m P. M. the first traces of an aurora were observed towards S.E. by S. The luminous appearance increased rapidly, spreading towards S.W.

6^h 40^m P. M. A rosy color appearing on the clouds in S.E. and S.W. by W.

6^h 50^m. Splendid aurora. Red streamers very bright, S.E., S.W., and W. by S. visible to an altitude of 50° or 90°. One very bright whitish streamer in S.W. by S. looking as if there were a thin red curtain before a beautiful white luminous curtain. Lower edge about 12° above the horizon. Well defined in S. by W. and S.S.W.; upper portion scarce visible at 45°. The folds of the luminous curtain and the red streamers, if produced, would probably meet one another about 10° S. of the zenith.

7^h 15^m P. M. Aurora fading away. Red patch in S.

7^h 20^m P. M. Red color disappearing from S. to S.W. giving place to white; at the same time the white in S.S.E. becoming reddish.

7^h 21^m P. M. Sky in south becoming very bright and white. Low bank of well defined cumulo-stratus 5° to 6° above the horizon.

7^h 23^m P. M. A well defined arch of white light 10° to 12° high above the bank of cloud before mentioned, extending from S.S.E. to W.S.W. being brightest in W.S.W.

7^h 30^m P. M. Very faintly red in S.E. Two pink streamers. Two whitish streamers, one in the zodiac, and the other through the cross.

7^h 34^m P. M. Faint rosy light in E.S.E. nearly as high as the zenith.

7^h 43^m P. M. White streamers in S.W. by W.

7^h 49^m P. M. A large patch of very bright light in S.E., white below, reddish above.

7^h 50^m P. M. A white luminous cloud appearing in S.W. About 30° high, below the southern cross, a rosy streamer in S.E. by E. very faint.

7^h 55^m P. M. The white and red light in S.E. increasing in brightness, yellowish white below, and red above. Top 40° high.

8^h 3^m P. M. Luminosity in S.E. almost gone, especially the red.

8^h 20^m P. M. Rosy arc from E.S.E. to W. by N., passing nearly through the zenith.

9^h 50^m P. M. Three red streamers in S.E. very bright, and several white ones in S.W.

12^h 15^m A. M. Bright broad streamers S.S.W. to S.W. partly covered with clouds.

12^h 40^m A. M. Luminosity in S. and S.W. 25° high.

2^h 15^m A. M. Luminosity from S.S.E. to W., brightest in S.S.W.

The magnetic disturbances continued with more or less intensity until 4 A. M. Aug. 30th.

During the whole of the 29th the instruments of the electric telegraph were disturbed to such a degree as to interfere with the working of the lines extending over New South Wales, Adelaide and Victoria. This effect was similar to that produced by atmospheric electricity.

E. Observations at Ballaarat, lat. 37° 36′ S., long. 143° 51′ E.

Aurora visible Aug. 29th at 6^h 45^m P. M. It gradually spread to the east and formed a magnificent arch, the colors of which were red, green and violet. The rays of light were distinct and beautiful. The southern portion of the sky was illuminated until 7^h 30^m sufficiently to cast a shadow.

F. Observations at Longwood, lat. 36° 54′ S., long. 145° 41′ E.

At 6^h 10^m P. M. Aug. 29th, an aurora appeared from a dusky line in the S.W. part of the horizon, which gradually ascended with a tremulous motion towards the zenith, assuming all shapes and varieties of color, from a pale red or yellow, to a deep vermillion, and extending to the N.E., serving to illuminate the earth, until its disappearance at 7^h 15^m P. M.

G. Observations at Sandhurst, lat. 36° 48′ S., long. 144° 24′ E.

Aurora very brilliant from 7 P. M., Aug. 29th, until a little after midnight.

H. Observations at Beechworth, lat, 36° 22′ S., long. 146° 52′ E.

Aug. 29th. Aurora visible for nearly an hour and a half, commencing about 5^h 45^m P. M., gradually increasing in beauty and brilliancy of tint until shortly before 7^h, when the rays became gradually indistinct, disappearing at about 7^h 15^m P. M. During the whole day the telegraph wires were strongly affected.

I. Observations at Sydney Observatory, lat. 33° 52′ S., long. 151° 12′ E., made by W. SCOTT.

The aurora was first noticed, Aug. 29th, at 7^h 20^m P. M. and continued visible for about half an hour, when it gradually faded away, and the sky became rapidly covered with clouds. I was in the act of observing a transit of the Pole star, when I was struck with the redness of the southern sky. On looking out I was surprised to find a considerable portion of the southern sky in a glow of red light, similar to that which sometimes precedes the rising of the sun. This red light formed a tolerably regular arch from E.S.E. to W.S.W., extending in depth from the south pole to within a few degrees of the horizon. There was a partial break to the S.S.W., and in some places there were radiating streams of light brighter and of a lighter red than the rest.

About 10 A. M., Aug. 29th, the wires of the electric telegraph were seized with an unaccountable fit of restiveness. They did not altogether refuse to work, but acted irregularly, the adjustment of the instrument altering so frequently that it was almost impossible to get any continuous message through. This state lasted until the evening, when the wires began to work better.

From the preceding observations, and from those which have been heretofore published in this Journal, it appears that the remarkable auroral display which prevailed throughout a large portion of the northern hemisphere from Aug. 28th to Sept. 4th, 1859, was accompanied by a display about equally remarkable in the southern hemisphere; and the periods of greatest brilliancy were nearly cotemporaneous in both hemispheres. In order to determine whether such a coincidence is a common occurrence, I have sought for some long and continuous record of the aurora in the southern hemisphere. The most complete record of this kind which I have found is that made at the British magnetic observatory at Hobarton, on Van Dieman's Island during the years 1841–48. These observations have been published by the British Government, and the first part of the following table contains all the instances of auroral exhibitions which I have been able to find in these volumes.

The second part of the table contains the corresponding observations made at New Haven by Mr. E. C. Herrick, who kept a careful record (negative as well as positive,) of all auroral phenomena from 1837 to 1853, except from Mch. to Sept. 1851.

The third part of the table contains auroral notices from the State of New York, as published in the annual Regents' Reports; and the fourth part of the table contains auroral notices from the Toronto meteorological observations.

I. *Observations of the Aurora at Hobarton, Van Dieman's Island,* lat. 42° 52′ S.
long. 147 27 E.
Magnetic Dip in 1845 . . . . -70° 35′·6.

Hobarton mean time, Astronomical reckoning.		Notices of Auroras.
Day.	Hour.	
1841. March 16,	17 *h.*	Slight appearance of Aurora.
March 22,	15	Faint appearance of Aurora.
May 17,	13	Slight appearance of Aurora.
July 20,	9	Aurora very brilliant in S.E.
Dec. 17,	11	Slight Aurora in south.
1842. Jan. 1,	11	Appearance of Aurora to the south.
Feb. 2,	9	Slight appearance of Aurora in S.W.
Feb. 18,	9	Appearance of Aurora in the south.
April 11,	9	Slight appearance of Aurora in the south.
April 13,	13–15	Aurora in the south.
April 14,	9	Aurora in the south.
April 15,	9	Aurora in the south.
May 16,	9–15	Aurora from S.E. to S.
June 13,	11	Faint Aurora in the south.
July 2,	7–11	Slight Aurora in the south.
Sept. 2,	13	Steady bright light in the S.
Dec. 31,	9	Slight Aurora in the south.

Table continued.

Hobarton mean time, Astronomical reckoning. Day.	Hour.	Notices of Auroras.
1844. April 16,		Aurora in the evening and night.
April 25,	9	Faint appearance of the Aurora.
1846. Sept. 22,		Aurora very brilliant throughout the night.
1847. April 20,		Aurora very distinct during the night.
April 21,		Aurora visible.
Sept. 24,		Aurora very bright.
Sept. 25,		Aurora visible.
Sept. 26,		Aurora visible.
Oct. 22,		Aurora visible and very brilliant.
Oct. 23,		Aurora visible.
Oct. 24,		Aurora still visible.
Dec. 20,		Aurora visible.
1848. March 24,		Aurora very distinct at night.
April 6,		Aurora very distinct at night.
Oct. 18,		Aurora visible.
Nov. 19,		Aurora visible.
Dec. 21,	10	Slight signs of Aurora to the south.

II. *Observations of the Aurora at New Haven*, lat. 41° 18′ N., long. 72° 55′ W.
Magnetic Dip in 1844 . . . . 73° 21′.

Date.	Notices.
1841. March 16,	Snowing.
March 21,	10·45 P. M., faint Aurora; 22d, cloudy.
March 23,	10 P. M., Aurora with streamers.
May 17,	Clear. No Aurora seen up to 10^{h} 15^{m}.
July 19,	10 P. M., Aurora with streamers; 20th, clear. No Aur.—10^{h} 15^{m}.
July 21,	10 P. M., Aurora.
Dec. 17,	Overcast.
1842. Jan. 1,	Clear. No Aurora up to 10^{h}.
Feb. 2,	Somewhat hazy. No Aurora up to 10^{h} 30^{m}.
Feb. 18,	Overcast.
April 11,	Aurora with streamers.
April 13,	Raining.
April 14,	10 P. M., Aurora reaching 20° altitude.
April 15,	3 A. M., Aurora reaching 45° altitude.
May 16,	Hazy: moonshine; no Aurora up to 10^{h}.
June 13,	Raining.
July 2,	Overcast.
Sept. 2,	8·30 P. M., Aurora with streamers.
Dec. 31,	Clear. No Aurora up to 11^{h}.
1844. April 16,	Overcast.
April 25,	Overcast.
1846. Sept. 21,	Aurora; 22d, 8 P. M., Aurora.
1847. April 20,	Overcast.
April 21,	Overcast.
Sept. 24,	Raining.
Sept. 25,	Raining.
Sept. 26,	Raining.
Oct. 22,	Overcast.
Oct. 23,	Overcast.
Oct. 24,	Raining.
Dec. 20,	5 A. M., grand Auroral display.
1848. March 24,	8 P. M., Aurora with streamers.
April 6,	9·30 P. M., Aurora with streamers.
Oct. 18,	Raining.
Nov. 19,	8 P. M., Aurora.
Dec. 21,	Snowing.

III. *Observations of the Aurora at the Academies in the State of New York.*
Magnetic Dip from 73° to 75°.

Date.			Notices.
1841.	March 16,		Aurora seen at Fredonia.
	March 22,		Aurora at Newberry, Vt.
	July 20,		Aurora seen at St. Lawrence.
1842.	Feb. 1,		Aurora seen at Cortland.
	April 11,		Aurora at Albany, Rochester, and many other places.
	April 12,		Aurora at Malone.
	April 14,		Aurora at Albany and many other places.
	April 15,		Aurora at Rochester and many other places.
	June 13,		Aurora at Ellisburgh.
	July 2,	10 P. M.,	Bright Aurora at several places.
	Sept. 2,		Aurora at North Salem.
1844.	April 17,		Aurora at Onondaga.
1846.	Sept. 21,		Aurora at North Salem and several other places.
	Sept. 22,		Aurora at Onondaga.
1847.	Oct. 23,		Aurora at Rochester and Casenovia.
	Oct. 24,	3 A. M.,	Brilliant Aurora at Rochester.
	Dec. 20,		Aurora at Hamilton and Mexico.
1848.	March 24,		Aurora at New York, Fredonia, and many other places.
	April 6,		Brilliant Aur. at Albany, Rochester, and many other places.
	Nov. 18,	morning.	Splendid Aurora at New York.

IV. *Observations of the Aurora at Toronto*, lat. 43° 40′ N., long. 79° 23′ W.
Magnetic Dip in 1845 . . . . 75° 15′.

Date.			Notices.
1841.	July 19,		Aurora from 9^h to 13^h.
	Dec. 17,	14^h.	Faint auroral light in north.
1842.	Feb. 18,		Rain and snow.
	April 10,	14^h.	Bright bank of auroral light in N.
	April 14,	14^h.	Brilliant Aurora.
	April 15,	8^h.	Aurora visible from 8^h to 14^h.
	July 3,	14^h.	Brilliant Aurora.
	Sept. 2,	9^h.	Faint auroral light at 9^h and 10^h.
	Dec. 31,		Snow.
1844.	April 16,		Auroral light.
	April 25,		Rain.
1846.	Sept. 21,		From 9^h to 17^h brilliant Aurora.
1847.	April 19,		From 13^h to 16^h Auroral light in N.
	April 21,		Rain.
	Sept. 24,		Rain.
	Sept. 25,		Rain.
	Sept. 26,		Rain.
	Oct. 22,	16$_h$.	Remarkable appearance of Aurora.
	Dec. 19,	17^h.	Aurora. Great magnetic disturbance.
1848.	March 24,		From 9^h to 12^h Aurora.
	April 5,		From 10^h to 15^h Aurora.
	Oct. 18,		Auroral light through the clouds.
	Nov. 19,		Slight auroral light.
	Dec. 21,		Snow.

Part first of the preceding table contains a list of 34 auroras observed at Hobarton. Part second of the table shows that in 11 of these cases an aurora was seen on the same day at New Haven. These observations were not strictly cotemporaneous, for Hobarton and New Haven being in nearly opposite longitudes, when an aurora was seen at Hobarton it could not be seen at New Haven on account of the presence of the sun. Moreover, the New Haven observations were chiefly made in the early part of the night; but in 11 cases an aurora was seen within about 12 hours of its appearance at Hobarton. In several cases when an aurora was seen at Hobarton it was cloudy at New Haven, and there were eight other corresponding cases in which an aurora was seen at some one of the Academies in New York, although not noticed at New Haven. In four additional cases an aurora was seen at Toronto, when none was recorded at New Haven or in the State of New York.

There remain then only 11 cases of auroras at Hobarton for which we do not find corresponding observations from one of these three sources in the northern hemisphere, and in eight of these cases the sky was overcast from New Haven to Toronto. The following are the dates of these auroras, and opposite to the dates I have placed notices of auroral or magnetic phenomena from some station in the northern hemisphere.

	Date.	
1841.	May 17,	Unusual magnetic disturbance at Greenwich, Eng.
1842.	Jan. 1,	Unusual magnetic disturbance at Greenwich.
	Feb. 18,	Unusual magnetic disturbance at Greenwich. Aurora at Christiana, Norway.
	May 16,	Unusual magnetic disturbance at Toronto and Greenwich.
	Dec. 31,	Magnetic disturbance at Greenwich.
1844.	April 25,	Unusual magnetic disturbance at Philadelphia and Toronto.
1847.	April 21,	Unusual magnetic disturbance at Greenwich.
	Sept. 24,	Aurora 9^h to 10^h at Greenwich.
	Sept. 25,	Unusual magnetic disturbance at Greenwich.
	Sept. 26,	Unusual magnetic disturbance at Greenwich. Aurora at Carlisle, England.
1848.	Dec. 21,	Aurora in Newfoundland.

It thus appears that in every instance when an aurora was observed at Hobarton, an aurora was seen on the same day in the northern hemisphere; or there were observed unusual disturbances of the magnetic instruments, indicating the existence of an aurora at no very remote station. So far then as a conclusion is authorized from so small a number of observations, we should infer that whenever an aurora is seen at Hobarton, where the magnetic dip is −70°, an aurora occurs at some place in the northern hemisphere as far south as where the magnetic dip does not much exceed 75°; in other words, an unusual auroral display in the southern hemisphere is *always* accompanied by an unusual display in the northern hemisphere. As any cause which affects the intensity of the magnetism at one pole of a magnet, usually affects the other pole, so an exhibition of auroral light about one magnetic pole of the earth, is uniformly attended by a simultaneous exhibition of auroral light about the opposite magnetic pole.

New Haven, May, 1861.

ART. XIII.—*Rock Oil, its Geological Relations and Distribution;* by Prof. E. B. ANDREWS, Marietta College, Ohio.

MY investigations have been directed chiefly to the oil of the coal rocks, and I propose in this paper to give some of my results.

The surface indications of petroleum are oil and gas springs. These springs are found scattered over a very large area.

It is doubtless well known to scientific men that there are, in the West, two distinct geological formations from which petroleum or rock oil is obtained. These are the bituminous coal measures and the Portage and Chemung groups, (the Waverly sandstone of the Ohio Reports). The Portage and Chemung rocks sweep around in the form of a quadrant from northwestern Pennsylvania into southern Ohio and south into Kentucky. Upon these rocks the famous oil regions of Pennsylvania and northeastern Ohio are located. The oil regions of western Virginia and southern Ohio, including a portion of western Pennsylvania, lie in the coal measures. Marietta, Ohio, may be regarded as near the centre of these extensive oil fields.

It is well known that in the manufacture of coal oil a large amount of vapor or gas remains uncondensed. The town of Newark, Ohio, has been lighted for several years by the surplus gas from the oil works there. While it is believed that the oil cannot be produced in the subterranean distillation without the production of gas, it is also reasonable to suppose that at the very low temperature at which this distillation must take place, the formation of gas necessarily implies the formation of more or less oil. Hence in our bituminous coal measures a gas spring doubtless indicates the existence of oil in the rocks below. The great majority of these gas springs are unknown, since they are seldom discovered, except when they appear in streams; and probably the same may be true of oil springs, since the soil would absorb the oil and in only a few cases would it be detected. I have assumed that the oil is the product of the distillation of bituminous strata, at low temperatures. This theory, which is a modification of the old one of distillation (at high temperatures), has recently been brought forward by Prof. J. S. Newberry and has received the sanction of many of our most eminent chemists. The chief objection to it is the fact that the coal, cannel and bituminous, in our oil regions gives no evidence of having lost any of its full and normal quantity of bitumen or hydrocarbons. For example, at Petroleum, Ritchie Co., Va., where strata have been brought up by an uplift from several hundred feet below, seams of cannel and bituminous coal appear, which, if judged by the standard of Nova Scotia or English coals, have lost none of their bituminous properties. The cannel coal although somewhat earthy, yields from forty to sixty gallons of oil to the ton.

The other theory, that the oil was produced at the time of the original bituminization of the vegetable or animal matter, has many difficulties in its way. If the oil were formed with the bitumen of the coal, we should expect that wherever there is bituminous coal there would be corresponding quantities of oil. This is not so in fact, for, as will be seen presently, there is no oil except in fissures in the rocks overlying the bituminous strata, and these fissures can be shown to have been made since the coal strata became bituminized. Again, upon this theory it will be difficult to explain the large quantities of inflammable gas always accompanying the oil. If it is generated exclusively from the oil, then we should expect to find the quantity of oil least where the gas springs have for ages been the most active, but at such places the oil, instead of being wasted, is the most abundant.

That the oil is accumulated in fissures in the rocks and that these fissures are more or less vertical there is abundant proof. The oil in the same neighborhood is found at very different depths. It is very seldom that two adjoining wells strike the oil at the same distance below the surface. The accompanying diagram shows the relative depths of oil wells on Burning Spring Run in Wirt Co., Va. It is evident that in this case the oil is not contained in anything like a horizontal reservoir. The famous Lewellyn well marked No. 2, struck an oil fissure at one hundred feet, while the Athens company's well marked 8, struck a large vein at two hundred feet. The oil in the first is said to stand at 41°, (Beaumé,) while that of the latter stands at 33°. On Duck Creek, Washington Co., Ohio, wells very near each other show a difference of ten degrees (Beaumé) in specific gravity.

1.

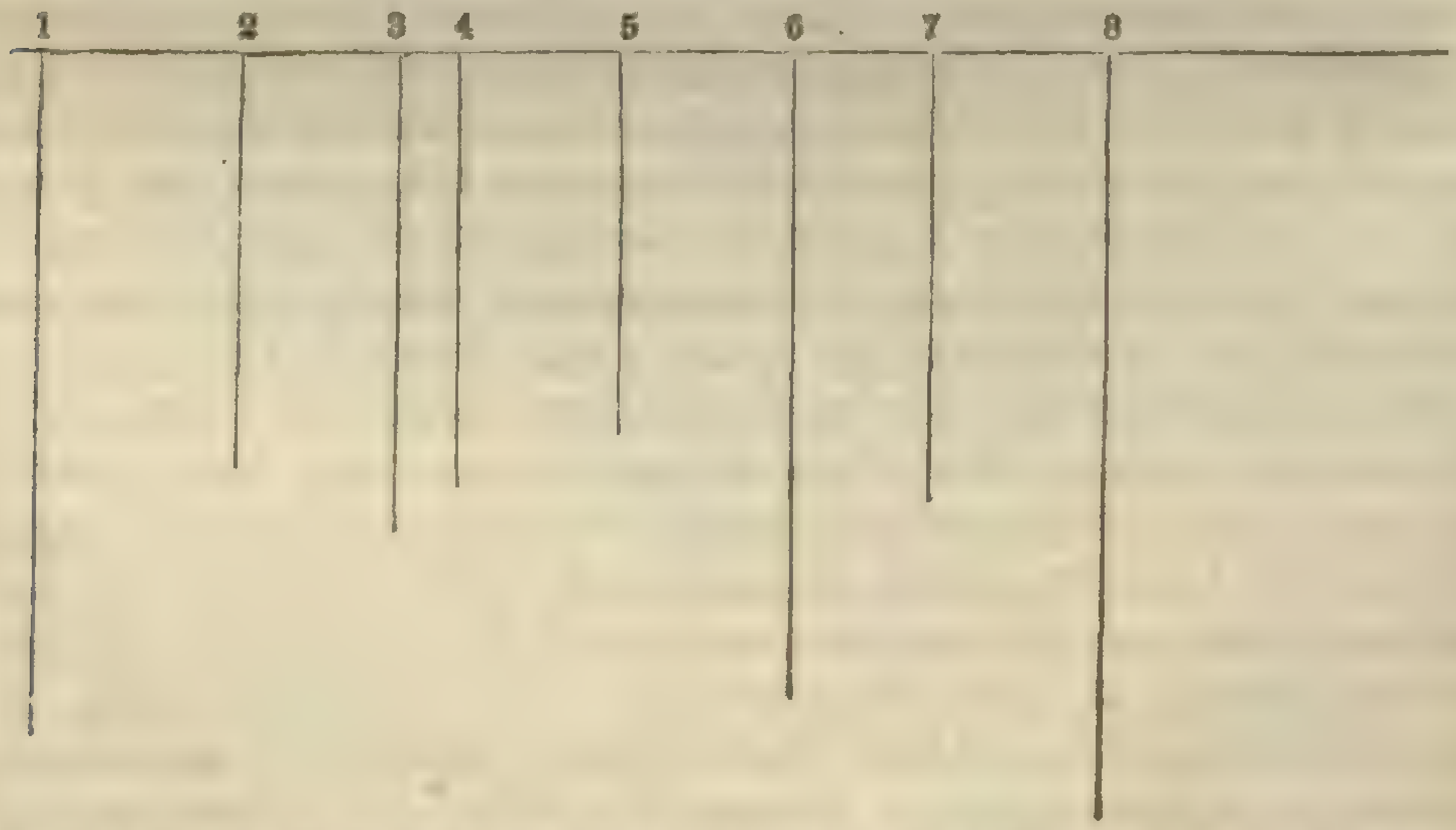
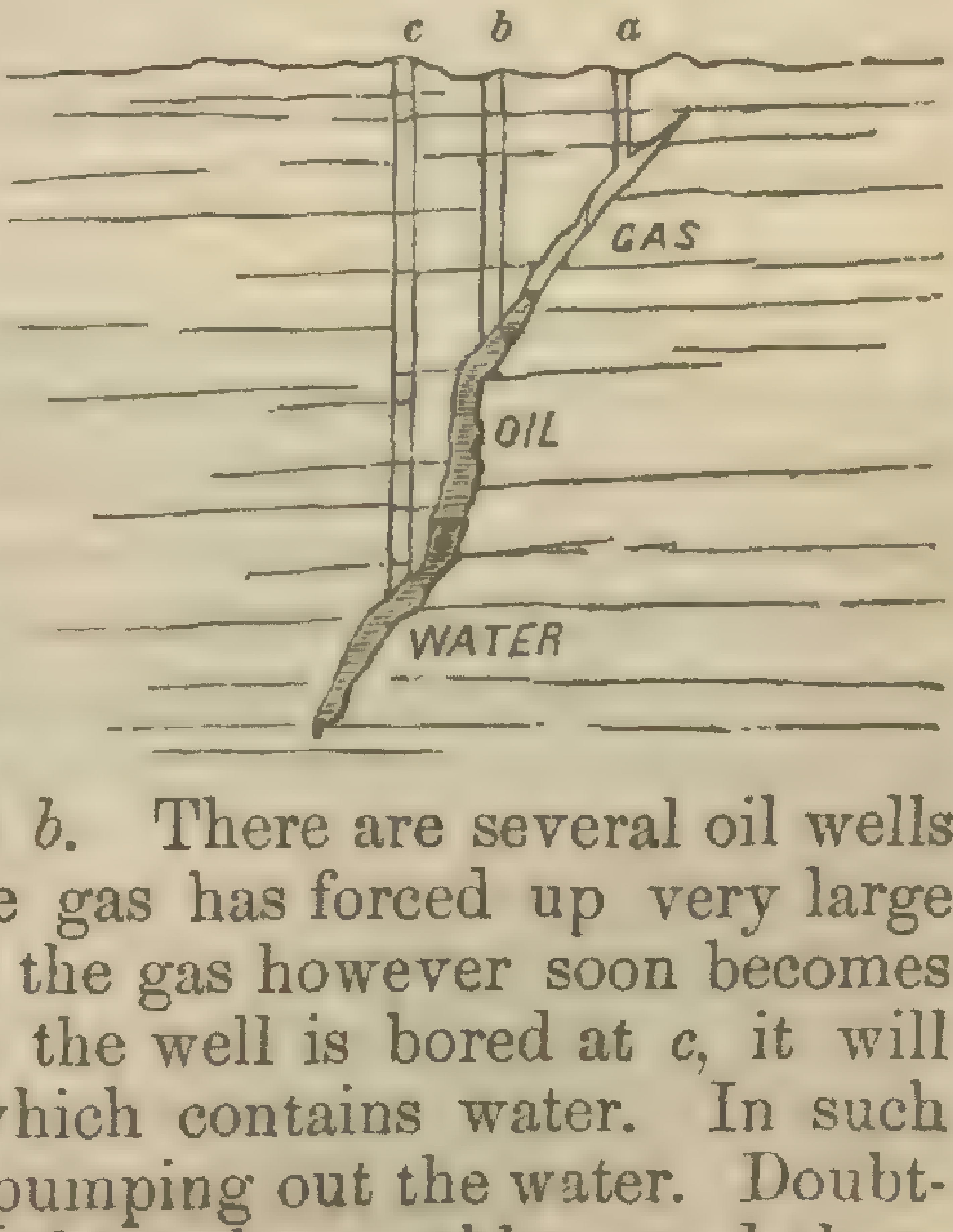


Fig. 1 shows the relative depths of the large wells in Burning Spring Run.

At Smith's Ferry, in Western Pensylvania, on the Ohio River, much of the oil is of a light straw color, while it is said other wells yield an oil of the more usual dark-greenish color. On the same lease of land and within six or eight rods of the well marked 8 in the previous figure, is a well two hundred and fifty feet deep. The oil from this well is not only different in its specific gravity from that in the other, but the deepest well contains fresh water, while the other contains salt water. From these and similar facts, it is evident that the oil is in distinct and separate fissures and that these are vertical rather than horizontal.

The contents of these fissures are generally water, at the bottom, oil floating upon the water, and gas filling the space above the oil. Where the gas finds an outlet through a crevice in the overlying rocks, there is produced a gas spring. When the water finds an outlet it carries the oil with it and an oil spring is the result. I have found oil springs high up on hill sides.

2.



An oil fissure may be struck at any point. The following figure will illustrate this. The letter *a* (fig. 2) shows the position of a well which strikes near the top of a fissure and in that part which is filled with gas. The gas rushes up but no oil. If again the well is bored at *b*, it will strike oil, and the gas pent up in the upper part of the fissure will force the oil up through the well *b*. There are several oil wells on Little Kanawha in which the gas has forced up very large quantities of oil. The action of the gas however soon becomes fitful and intermittent. If again the well is bored at *c*, it will strike that part of the fissure which contains water. In such case oil can be obtained only by pumping out the water. Doubtless many good oil wells are thought to be worthless, and abandoned because they contain at first only water. If bored at the point *c*, in the water part of the fissure, water alone is to be expected until the pump has been used. There may be floating upon the water, higher up in the fissure, a large quantity of oil.

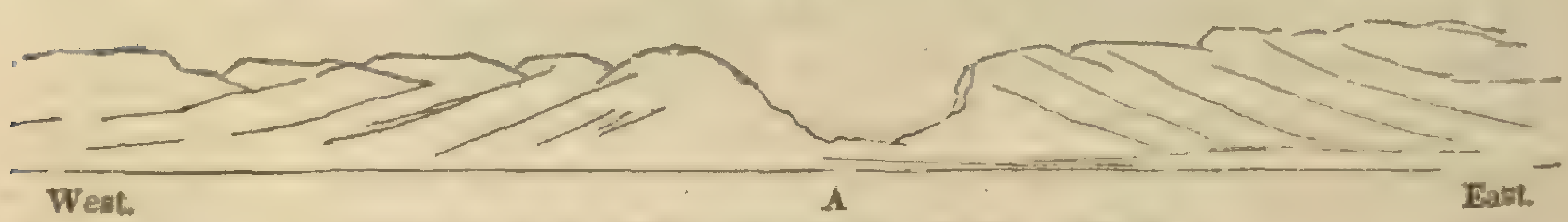
If the oil is found in fissures in the rocks, it is natural to suppose that in those places where the fissures are the most numerous and largest, the oil would be formed in the largest quantity. This antecedent probability is fully verified by the facts. The rocks of Western Virginia and Southeastern Ohio may be divided into three classes, *those which are almost entirely horizontal, those which have a dip of from fifteen to forty feet in the mile, and* *those which are broken and dislocated by an uplift.* The strata from the Ohio River at Parkersburg up the Little Kanawha to within a few miles of the great oil wells are very nearly horizontal and probably contain few fissures except such as may have been produced by the drying and shrinking of the rocks. There is not to my knowledge a single productive oil well in that region although a large number of wells have been bored. The compact and unbroken clay shales and other strata rest upon the deep bituminous strata and furnish no spaces through which the oil vapor could rise. Probably no such vapor is formed.

On the Great Kanawha River, at Pomeroy and vicinity on the Ohio River, in Athens, Morgan, Noble, Washington and other counties in Ohio, located on the coal measures, the rocks have more or less dip and contain, as a probable result of the uplifting force, many fissures. These counties all furnish oil—Noble and Washington in considerable quantities. The salt wells on Great Kanawha, at Pomeroy on the Ohio, on the Hocking and Muskingum Rivers and on Duck Creek, revealed more or less oil.

But it is in regions where the strata have been the most disturbed and where the fissures are the most numerous, that the most oil is found.

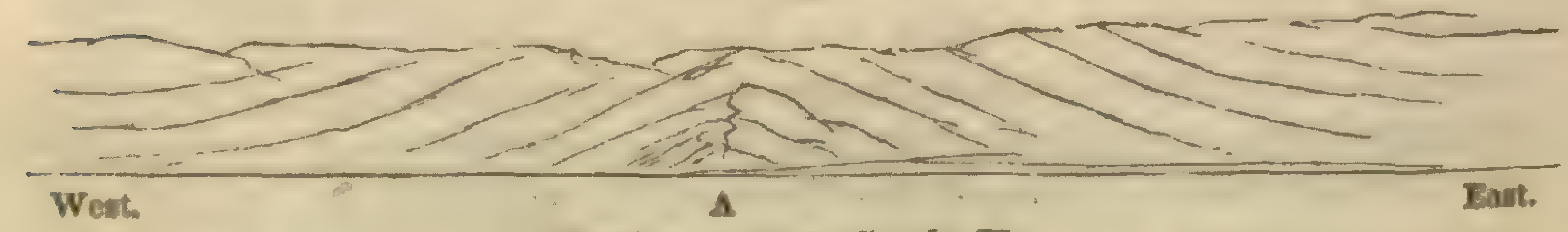
I have recently traced a most interesting line of uplift and dislocation from the eastern part of Washington county, Ohio, to beyond the great oil wells on the Little Kanawha River. The direction of it is nearly north and south. It makes an angle of about 40° with the general course of the Alleghany Mountains. As seen in Ohio it presents a well marked anticlinal axis but with the eastern slope more steep than the western. Near the anticlinal lines at A, fig. 3, are oil and gas springs. A few miles south, on Cow Creek, Va., it is seen as represented by fig 4. At the anticlinal line are gas and oil springs. Fifteen or twenty miles farther south, near Petroleum, Ritchie Co., Va., the uplifting force has been greater and the strata have been broken apart

3.



Section on the Ohio in vicinity of Newell's Run.

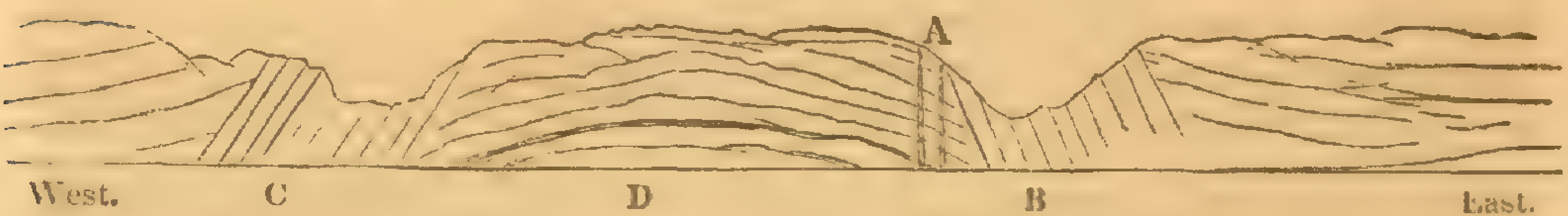
4.



Section on Cow Creek, Va.

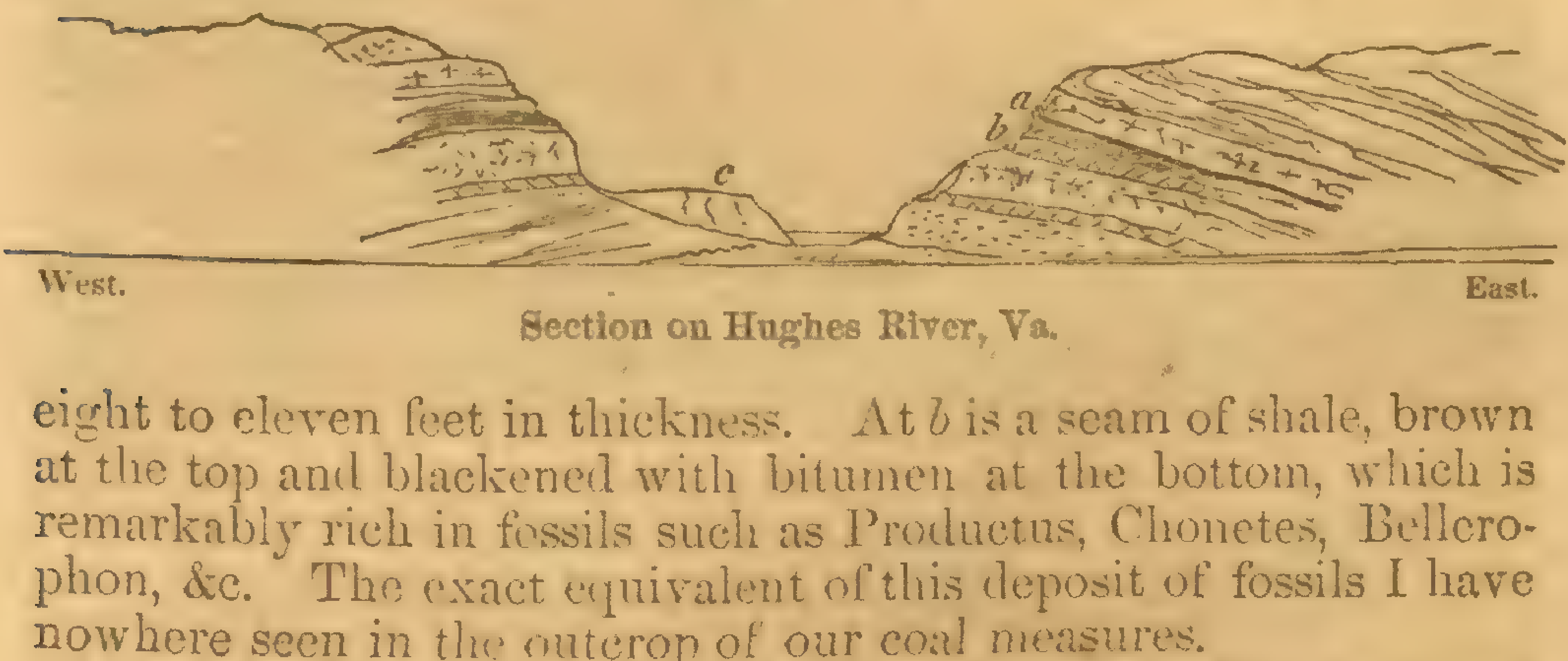
and now stand on each side at an angle of about 50°. They are represented by the inclined strata B and C in fig. 5. At D, are seen strata from below, which have been forced up and wedged in between B and C. They have been bent by the great force to which they have been subjected. These strata contain seams of cannel and bituminous coal and are altogether new to me. At A, just within the steep rocks on the east are gas and oil springs and some productive oil wells. At B, is seen the mouth of Oil Run and at C, the mouth of Laurel Fork of Goose Creek, where the rocks of the western dip are finely exposed in a railroad cutting. A few miles further south and on the line of this geological disturbance we find, near where Hughes River crosses the uplift, many new and interesting strata, which have been lifted up from considerable depths. Some of these are shown in fig. 6. At *a* we find a seam of light colored compact flint from eight to eleven feet in thickness. At *b* is a seam of shale, brown at the top and blackened with bitumen at the bottom, which is remarkably rich in fossils such as Productus, Chonetes, Bellerophon, &c. The exact equivalent of this deposit of fossils I have nowhere seen in the outcrop of our coal measures.

5.



Section near Petroleum Station, N.W. Va. R.R.

6.

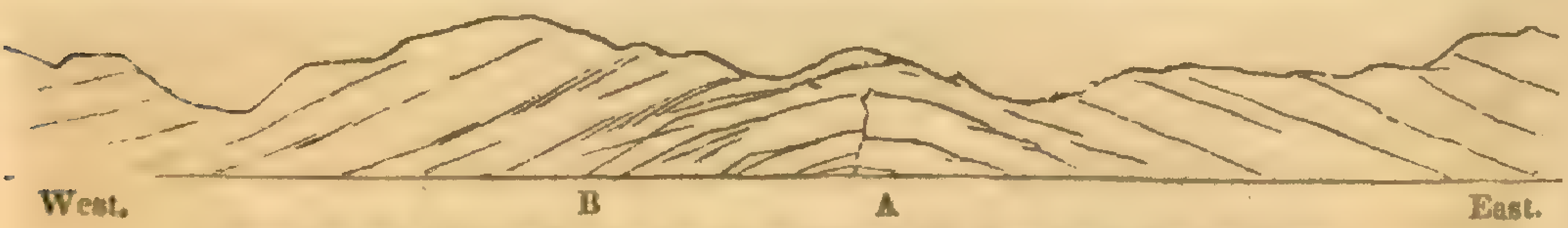


Section on Hughes River, Va.

At *c*, is shown the alluvial bank of Hughes River, out of which large quantities of petroleum have in former years been taken. The oil came up through a fissure in the underlying rock and saturated a stratum of sand and gravel. This stratum was laid bare by removing the superincumbent earth and the oil worked out by hoes from the water and sand. This locality, next to the old Pennsylvania Oil Springs, has probably furnished more oil than any other locality in the country. A few wells have been bored in this region, but with what success I did not learn, nor had I time to investigate the question whether they were bored in that part of the geological disturbance, where the largest number of fissures would be found.

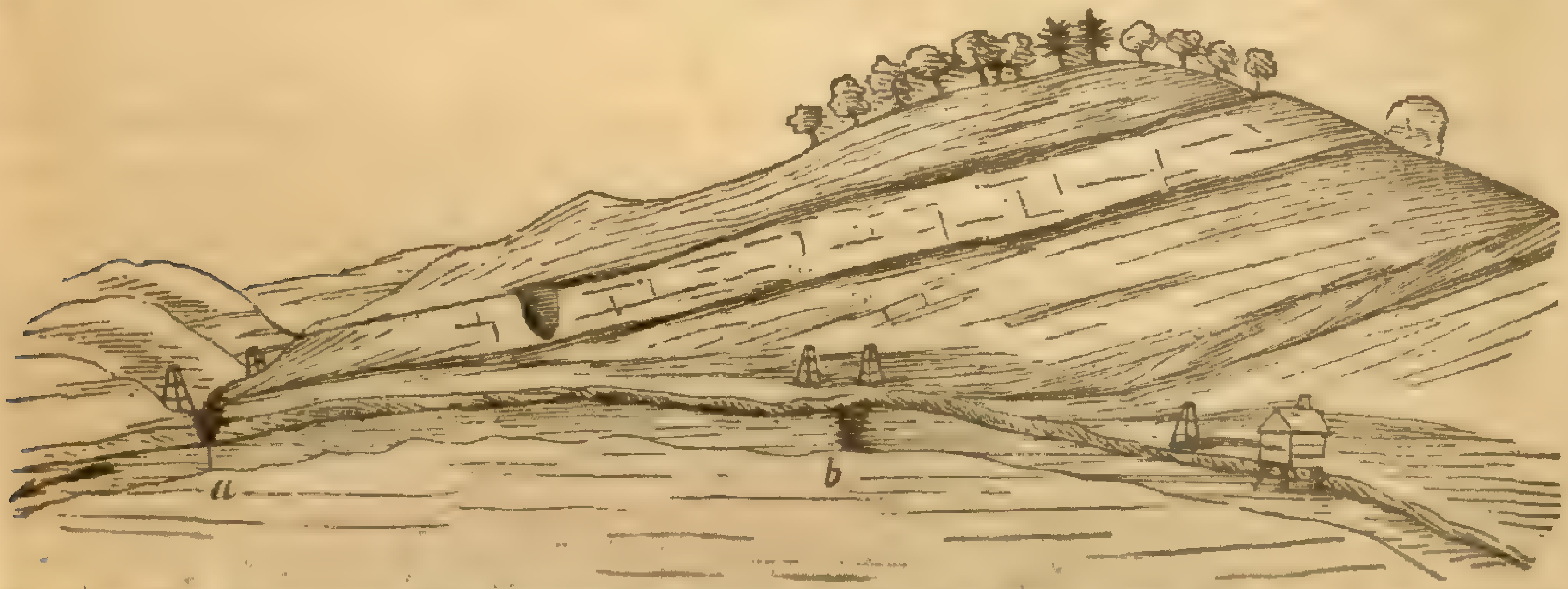
Between this point and the Little Kanawha River, the anticlinal line is easily traced, the rocks inclining to the east and to the west at angles varying from 28° to 8°. The rocks are well exposed on the heads of Standing Stone Creek, and at other points. On the Little Kanawha, a section would be represented by fig. 7. The anticlinal line, or centre of the uplift is seen at A. Near A and extending towards B are the great oil wells. They are exactly where the largest number of fissures would be made by the uplifting force. The accompanying figures 8 and 9

7.



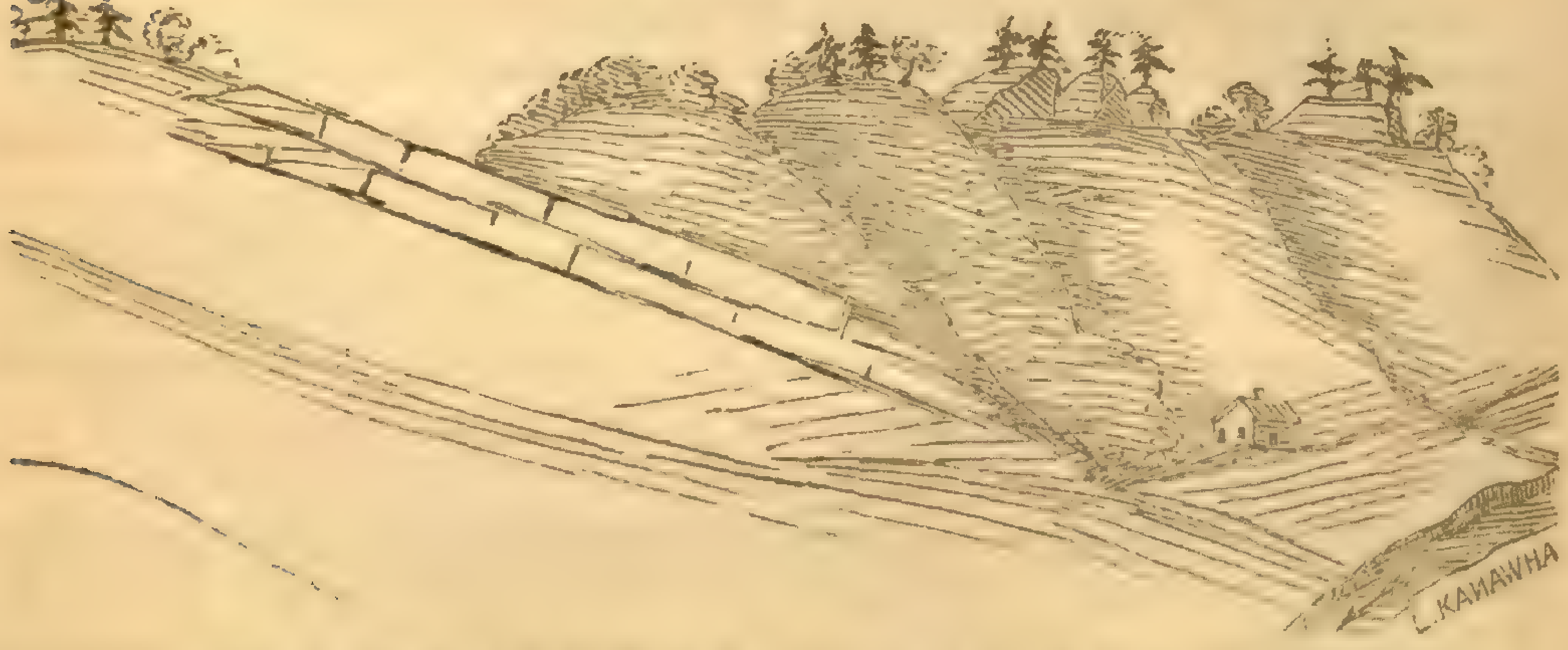
Section on Little Kanawha in vicinity of Burning Spring Run.

8.



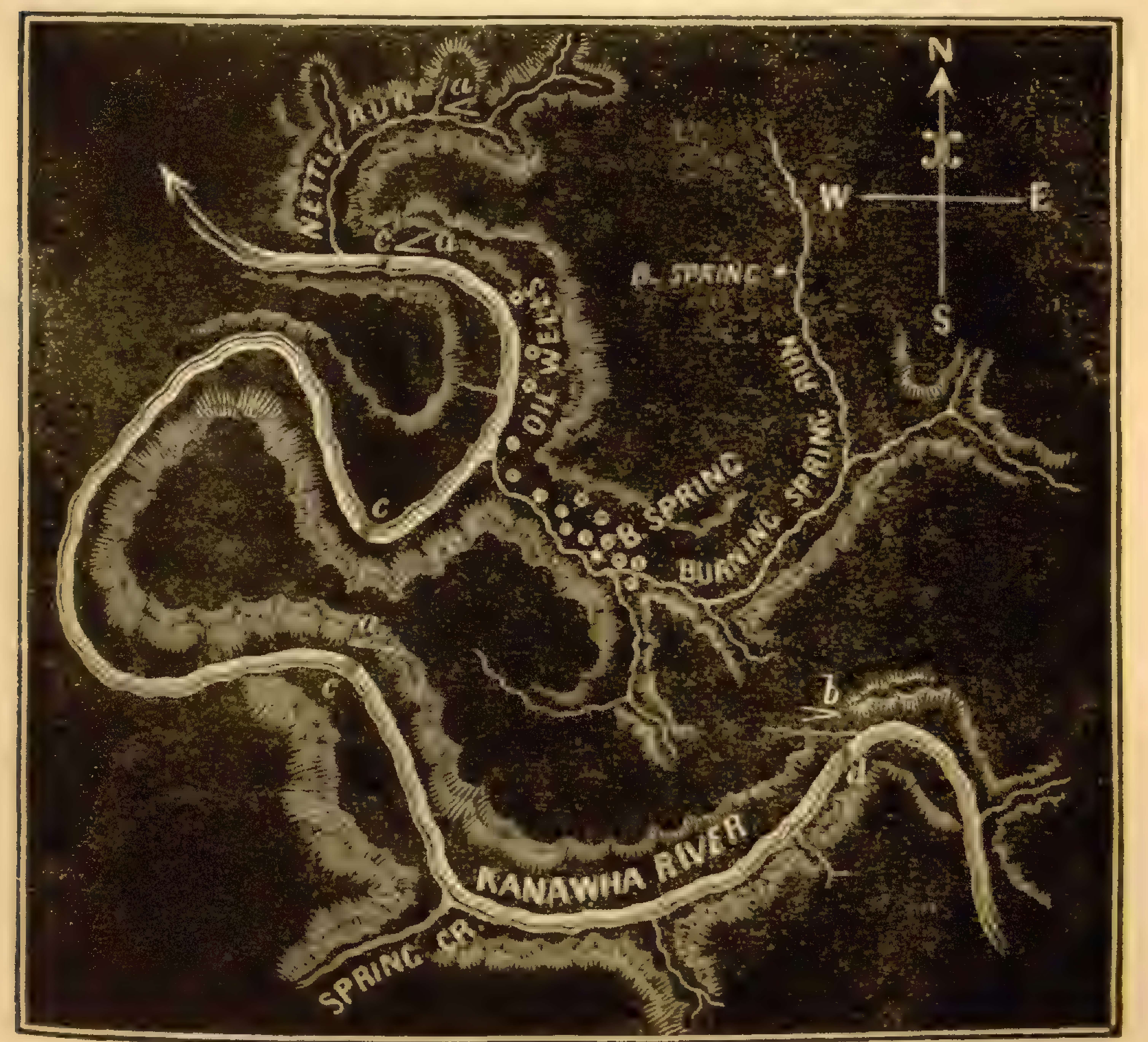
Sec. on Lit. Kanawha R. above the mouth of Nettle Run. Dip West. *a* mouth of Nettle R., *b* ledge of rocks in the bed of river.

9.



Sec. of Eastern dip ¼ mile E. of Burning Spring Run. Dip 11°.

show the western and eastern slopes of the anticlinal. To the intelligent aid of E. H. Moore, Esq., of Athens, Ohio, I am indebted for the following map of the oil region of Little Kanawha. The directions and courses were taken accurately, the distances only approximately. It is so far as I know the only map of that district. The wells are on the north bank of the river and on a little tributary called Burning Spring Run. The hills in this region are very high. The valleys are all valleys of erosion. The three angles marked *a* indicate the position of the western slope of the rocks. On Nettle Run, the dip is 21°, at the other points, somewhat less. At the points marked *c* are ledges of rock, seen in the bed of the river at low water, produced by the harder of the inclined strata.



Map of Little Kanawha Oil Region, Wirt Co., Va.
Scale about 1½ inch to a mile.

At *b* the angle indicates the dip of the strata to the east. The rocks are there inclined at an angle of 11°. At *d* another ledge of rocks is seen in the bed of the river made by hard strata in their eastern dip. The central line or axis, between the two slopes, runs nearly north and south, and along this line the productive wells are found. Thus far, the oil is obtained only in a narrow range extending north and south, and little more than one hundred and twenty rods wide. On the west of this range many wells have been bored without success. The exact limit of the oil on the East is not yet known,—but wells, now being bored, will soon determine it. The oil fissures are struck at different depths, as has been already shown, consequently there is no such thing as an "oil rock" as many suppose. The oil is found in any kind of stratum. Each oil fissure doubtless extends vertically or nearly so, through many different strata.

These wells have been unparalleled for the quantity of oil produced. Many of them, when first bored, poured out the oil in torrents, the oil being forced up by the pressure of gas. Hundreds of barrels were obtained from a well in a few hours. The Camden, Lewellyn, Weare and Harper wells and doubtless others, are fine illustrations of this. They yield a marvellous quantity of oil. In many wells the oil is entirely free from water and passes directly from the well into barrels for shipment. The oil varies in specific gravity from 32° to 42° (Beaumé.)

The oil is evidently the accumulation of long ages. The valleys of erosion which cross this line of uplift in almost every direction, and which have been produced by the drainage of the rains falling upon the surface, show that the uplift and consequently the fissures underneath, have existed for a vast period of time. It is therefore probable that during this long period the work of accumulation has been going on. If this is true, it will follow that when a fissure is once exhausted of oil, it may well be abandoned, as it will take a geological period to refill it. The original indications of oil in this region were, as I learn, only the gas springs, of which there were two on Burning Spring Run. Both of them are now destroyed by wells that have taken away the gas. A salt well bored at the mouth of Burning Spring Run, many years ago, yielded a large quantity of oil and it was this salt well which caused attention to be directed to this locality, now so famous.

I have thus shown some of the geological relations of the oil here in the upper part of the coal measures. The *horizontal* strata in Wood and Wirt counties, Va., are, I think, the highest in the series. Containing few fissures, the chances of finding oil in them is necessarily very small.

The *inclined* rocks with a dip of from fifteen to forty feet in a mile, contain more or less fissures and consequently yield more or less oil. It is in these inclined rocks that the productive wells on Duck Creek, O., at McConnelsville, Pomeroy, &c., are obtained.

In the *broken* rocks, as found along the central line of a great uplift, we meet with the largest quantity of oil. It would appear to be a law, that the quantity of oil is in a direct ratio to the amount of fissures. By this law, the great wells on Little Kanawha are easily explained. With so much room for expansion, the hydro-carbons of the coal and bituminous strata underneath have risen in the form of vapor and during long geological ages have been filling the fissures with oil.

Since writing the above, I have examined the Report of the Geol. Survey of Kentucky, vol. iii, and find that the oil region which lies in Cumberland county and in several adjoining counties, is probably situated upon a line of peculiar disturbance. Mr. Owen states that "the principal axis of disturbance, already mentioned, which passes in a southwest course through Lincoln, Casey, Russell and Cumberland into Monroe county, probably crosses the Cumberland (river) at the Riffle, near the Turkey River Bend, where a dip of about 4° was observed at the head of the Riffle in a direction of 50° east, while a reversed dip, north 50° west, at about the same angle was noticed near the foot of the Riffle." A careful examination might show that the fissures containing oil in this region are to be found chiefly along the line of this disturbance.

Of a locality in Cumberland county, Mr. Owen thus writes: "On Crocus Creek, the blue limestone dips 2° to the northeast; a short distance up Puncheon Camp, the slate dips at an angle of about 1° in a course south 20° west, while not far off, on the Creek, it dips with about the same angle in a course north 10° to 20° east. Hence it is evident that the dip is very irregular." On Crocus Creek and on other streams in that region large quantities of oil were found many years ago in boring for salt water. From all I can learn, without visiting the region, the oil is found chiefly in the blue limestone and below the black shale. The oil is light in its specific gravity, while oil from similar locations in the geological series, in Ohio, is very thick and tar-like.

The oil from the coal measures varies in specific gravity from perhaps 20° to 52° (B.) Oil from Pomeroy, O., standing at 51° B. burns freely, and with brilliant flame, without distillation.

The heavier oils, such as are not profitable for distillation, afford a very superior lubricating oil. Hence the demand for heavy as well as light oils will doubtless be very great.

Marietta, Ohio, May 20, 1861.

Art. XIV.—*On the Crystalline Form of the Hydrate of Magnesia from Texas in Pennsylvania;* by George J. Brush.

The crystalline form of *Brucite* ($\dot{M}g\dot{H}$) was first studied by Prof. Dana, and shown by him to be rhombohedral,* the specimens examined were minute crystals collected by the writer in the year 1852 at *Low's* Mine in the town of Texas, Pennsylvania. Since then crystallized *Brucite* has been found in great abundance at *Wood's* Mine in Texas; the crystals vary from a microscopic size to sometimes two or three inches in diameter, and crystalline plates with rough prismatic faces of a much larger size are of frequent occurrence. The crystals are generally thin hexagonal plates with rhombohedral planes almost obliterating the prism; occasionally, however, the prism is oblong, the length being two or three times the breadth; they are symmetrically modified hexagonal prisms, and cannot be referred to any but the rhombohedral system.

In a recent article in the *Journal für praktische Chemie*, lxxxii, 368, R. Hermann describes the Texas mineral under the new name *Texalite*, and attempts to show that its form is monoclinic, and that hydrate of magnesia is consequently dimorphous. To establish this important conclusion Hermann gives four crystallographic measurements (approximative, doubtless, as only full degrees are noted), besides an examination of the optical properties of the mineral, and an analysis. The analysis proves, what has been already so well known, that the Texas mineral is hydrate of magnesia free from carbonic acid, and containing a small amount of manganese. The optical examination was made by Dr. Auerbach, and in his communication, quoted by Hermann, he distinctly points out the fact that with polarized light the mineral has the properties belonging to the *rhombohedral* system; the only point upon which the supposed dimorphism rests, then, is the measurement of the angles. Hermann finds that the angle between the basal plane and *four* similar lateral planes is 90°; this is sufficient to prove that the crystal cannot be monoclinic, for this angle corresponds to a right prism, and is an impossibility in the monoclinic system. Besides the basal and the four lateral planes here referred to, the crystal described by Hermann contained two other planes on the acute angles of the four-sided prism, making with the basal planes, the angles, according to his measurements, 115° and 157°. The specimen examined by Hermann was most probably one of those distorted crystals so frequently met with in micaceous minerals. In some hundred or more specimens from this locality which I have examined, the crystals are distinctly hexagonal, with the rhombohedral planes R and $-\frac{1}{3}R$, in some cases entirely obliterating the prism. The angles measured were as follows: $O:R$ 119°—120°, $O:-\frac{1}{3}R$ 149° 40′, $I:I$ 120°, $O:I$ 90°.

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* This Journal, [2], xvii, 83.

The proof of the rhombohedral character of the mineral is therefore abundantly demonstrated. The fact that Dr. Auerbach's examination of the optical characters of Hermann's mineral referred it to the *rhombohedral* system, would naturally have led one to be cautious in drawing the conclusion that it was dimorphous. This supposed anomaly between the optical characters and the crystalline form is entirely set aside by the facts here presented; we may consequently safely conclude that the assumed dimorphism does not exist, and that the so-called *Texalite* is nothing more than ordinary rhombohedral *Brucite.*

Yale Analytical Laboratory, May 23d, 1861.

Art. XV.—*Correspondence of Jerome Nicklès, dated Nancy, April 13th*, 1861.

Academy of Sciences—Distribution of Prizes.—The session of the 23d of March last commenced by the distribution of prizes, and closed with a eulogy of Legendre the geometrician, pronounced by Elie de Beaumont.

The Astronomical Prize was divided between five observers who have discovered planets in the course of the year 1860.

These astronomers laureate are:

Luther of Bilk, who discovered *Concordia* March 24, 1860; Goldsmith, who discovered *Danaë* at Chatillon near Paris, Sept. 9, 1860; Chacornac, who discovered a planet not yet named, at Paris, Sept. 12; Ferguson, who discovered *Titania* Sept. 14–15 at Washington; Forster and Lesser, who discovered *Erato* Sept. 14–15, at Berlin.

Concordia is the first of the five telescopic planets discovered in 1860. It is remarkable that the other four were all discovered in September, within an interval of five days. Erato was seen by Forster and Lesser while searching for the planet which Chacornac had discovered Sept. 12.

Lescarbault was not included in this number, his *observation of the passage of a planet across the sun's disk*,* March 26, 1859, not having been confirmed by other observations.

The Cuvierian Prize.—This is a triennial prize designed to reward researches in the different branches of Natural Science to which Cuvier was most devoted. It has been awarded three times. The first time to Agassiz for his work on *Fossil Fishes*; the second to Müller of Berlin; the third to Owen. Now this prize has been awarded to Léon Dufour of St. Sever (Department of Landes), for his patient researches in natural history prosecuted for more than half a century. Following in the footsteps of Swammerdam and Réaumur, Léon Dufour has been occupied mostly with the study of the organization and habits of insects.

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* See this Journal, [2], xxix, p. 415.

In a long series of anatomical monographs he has shown the general characteristics of the interior structure of all the principal representatives of this great division of articulate animals. Residing since 1814 in a small village at the foot of the Pyrenees he has devoted to anatomical investigations all the time which his profession of medicine left at his disposal, and even now at the age of more than 80 years, it is in researches of the same kind that he fills up the leisure of a green old age. The love of science has always been his only motive and the numerous services which he has rendered to zoology have brought him neither riches nor honor.

Among the works presented for the Cuvierian prize the Academy has noticed those of the American Zoologist, Girard, upon the *Ichthyologic Fauna of the Western Regions of the New World, and upon the Reptiles collected during the voyage of Capt. Wilkes in the Antarctic Seas.* This work, says the Commission,* is not unworthy to take a place with the beautiful and important works with which Mr. Dana, the principal Naturalist of that Expedition, has enriched science, and the Commission consider this a fitting occasion to testify publicly the satisfaction which they experience in the zoological studies undertaken in America, and that so great an undertaking finds there such powerful encouragement.

Chemical Prize.—The 1st prize of 3500 francs has been awarded to Berthelot "for his researches in Chemistry relating to the production, by synthesis, of certain chemical compounds existing in living bodies." The 2d prize of 2000 francs was awarded to Dessaignes "for the production of succinic, aspartic, hippuric, aconitic, fumaric and racemic acids, by the transformation of gelatine sugar."

The Report adds the following: "In making mention, at the present session, of the remarkable labors of Pasteur, the section of Chemistry wish to reserve the liberty of considering them in their full extent hereafter, both in regard to the past and to the future."

It is here proper to say that Pasteur, who has especially shared the prizes of the Academie des Sciences, will next year have a new prize of 6000 francs for the same class of labors which have been recompensed so frequently and in all forms.† This is a consoling circumstance proving that if, as the Commission of the Cuvierian prize say, there are savans whose labors have conduced neither to riches nor honor, the counterpart is equally true in France.

Prize in Experimental Physiology.—This prize was taken by Stilling (of Cassel) for his great work upon the "Structure of the Spinal Marrow." Honorable mention was accorded to Philipeaux and Vulpian for their experimental Researches, "*Sur la régénération des nerfs séparés des centres nerveux:*" and to Faivre for his work, "*Sur la modification qu' éprouvent après la mort, les propriétés des nerfs and des muscles chez les grenouilles.*"

Among the *medical prizes* we may mention that given to Devaine for the "*Traité des Entozoaires et des maladies vermineuses de l'homme et des animaux,*" a work which compared with what had previously appeared, forms an era in science and in medicine so far as they are concerned with the intestines. A prize was also decreed to Bergeron for his treatise upon *Stomatite ulcereuse des soldats* (soldier's sore mouth). This is a contagious disease which may be reproduced with some modifications by inoculation; this the author proved upon himself. According to documents collected by him it appears to be demonstrated that the appearance of ulcerated mouth was epidemic in the French army near the end of the eighteenth century. It happened that of all the great armies of Europe, those of Portugal and Belgium together with the French army, were the only ones in which this disease was observed in the form of an epidemic. The crowding of soldiers in the camp, in the barracks, and in the body guard, appears to have been the principal cause of the development and propagation of this affection. Bergeron has introduced the chlorate of potassa in the treatment of this malady. This method shortens the period of the disease, and if employed in the commencement it frequently obviates the neccessity of sending the soldier to the hospital.

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* Composed of Messrs. Flourens, Elie de Beaumont, Geoffroy St. Hilaire, Serres' and Milne Edwards (reporter.)

† See this vol., p. 1, for a notice of Pasteur's recent labors.

Among the Doctors Laureate of the Section of Medicine are also Doctors Turck, and Czermack, of Vienna, for the perfection which they have given to the *laryngoscope;* and Marey, for an instrument which registers the beats of the pulse. It is known that to observe the pulsation of the artery it is necessary to make considerable pressure upon the vessel. Recently Vierordt made experiments of this sort with his *sphygmograph.* This instrument, which caused serious errors, has been gradually perfected by Marey, who subjects the artery to elastic pressure which can be graduated at will according as the pulse is more or less depressed, that is to say, in proportion to the greater or less tension of the blood in the artery. This elastic instrument receives from the vessels alternate motions of elevation and depression, transmitting them to a lever which magnifies them and expresses faithfully every motion on an enlarged scale.

Subjects proposed for prizes next year.

The following are some of the subjects announced for the grand prizes in physical sciences for next year:

1st. Comparative anatomy of the nervous system of fishes.

2d. The study of vegetable hybrids in reference to their fecundity and the perpetuity or non-perpetuity of their characteristics.

3d. The changes which take place during germination in the constitution of the tissues of the embryo and perisperm and in the substances enclosed in these tissues. Also to attempt by reliable experiments to open a new course of investigation upon the "generations called spontaneous."

Numbers 1 and 2 should be presented before Dec. 31, 1861.

No. 3 should be presented on or before Apr. 1, 1863. Each prize consists of a gold medal of 3000 francs.

Medical prizes.

1st. History of the Pellagra. (*Scorbutus Alpinus* or *Elephantiasis Italica.*)

2d. The application of electricity to therapeutics.

Each prize is one of 5000 francs.

Besides these there will be prizes for discoveries or inventions relating to the healing art; prizes will also be given to those persons who shall find means of rendering an art or a trade less unhealthful.

Grand Surgical Prize—For the preservation of limbs by the preservation of the periosteum.—This is a prize of 20,000 francs, to be awarded, if there is a suitable occasion, in 1866. The following is an extract from the explanation of the object for which this prize is offered as published by Flourens.

"Numerous facts have proved that the periosteum has the power of producing bone. Recently some remarkable facts in human surgery have shown that very extended portions of bone have been reproduced by the periosteum which remained. The time appears to have arrived to call the attention of surgeons to a great and novel study which is interesting at once to science and to humanity. Those who engage in it will not forget that their labor is at once practical and that they are laboring for man, and that it draws no less upon their respect for humanity than upon their intelligence."

The Academy had decided that the prize should be 10,000 francs. When informed of this decision the Emperor, fully appreciating the benefits to be derived from such progress in surgery, immediately caused a communication to be sent to the Academy that the prize should be doubled.

The prize will therefore be 20,000 francs. The essays, written in French, should be presented to the Secretary of the Institute before the 1st of April, 1866.

Employment of Glycerine in Surgery.—The first application of glycerine in the treatment of disease appears to have been made in England about the year 1846. Having been first employed in certain diseases of the skin, it was applied by Dr. Demarquay in the treatment of wounds, ulcers, and certain affections of the genital organs. It was afterwards successfully applied to the dressing of wounds and even to those which had become painful. In an epidemic of hospital gangrene which occurred at the Hospital St. Louis at Paris after having employed without success lemon juice, nitric acid, and the red oxyd of iron, Demarquay made use of glycerine with a success surpassing his expectations.

Antagonism of the poisoning effects of strychnine and curare.—We have previously noticed* experiments on this point made by Dr. Vella of Turin. The author has shown that the curare destroys the effects of a dose of strychnine which would produce death when taken alone into the stomach or when injected into the veins. For by exhibiting together the curare and the strychnine, either separately or previously mingled, far from increasing the poisonous action of these substances, it is possible to neutralize them and cause their effects to disappear. The two substances do not exercise any chemical action upon each other, from whence it appears that the antagonism of their poisònous effects results from action wholly physiological.

An unpublished memoir by Lavoisier.—The works of Lavoisier are to be published at the expense of the government, and under the auspices of the minister of public instruction. Dumas, who has undertaken to edit them, has found many of his writings hitherto unpublished, one of which has just been published in an extremely interesting volume entitled, "*Leçons de Chimie professées en* 1860, *par MM. Pasteur, Cahour, Wurtz, Berthelot, Sainte Claire Deville, Barral et Dumas.*" Each of these savans has prepared a lecture upon a subject of his own selection. We shall notice these subjects further on in the bibliography—we only state here that the lecture by Dumas is designed to make known two unpublished documents which he has discovered, the one written by Lavoisier, the other by Leblanc the inventor of the artificial soda.

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* Vol. xxix, 269, March, 1860.

The paper of Lavoisier relates to the chemical statics of organized beings and is at least fifty years in advance of the scientific knowledge of his times.

We all remember the sensation occasioned at the appearance in 1840, of a work of Dumas and Boussingault, entitled, "*Leçon sur la statique chimique des étres organisés,*" and which was so brilliantly expounded by Dumas at a session of the Faculty of Medicine of Paris. But the harmony which was thus announced—the discovery of which was the fruit of twenty years labor—this brilliant discovery Lavoisier had long before foreseen. Dumas says, "Six years before the publication of *La statique chemique des étres organisés*, I received from the Academie des Sciences at the instance of the Minister of Public Instruction, the appointment to collect the information necessary for giving advice in relation to an edition of the works of Lavoisier. On that occasion Arago made known to us the existence of the records of the laboratory of Lavoisier, then in his possession, and also of writings of this great man remaining in the hands of his family. These papers having been communicated to me I proceeded to a careful and attentive examination of these noble relics."

It was quite recently while examining the administrative papers of Lavoisier, seeking for information in regard to the prize founded by the Academy in reference to the decomposition of sea salt, for the purpose of extracting soda, that Dumas discovered a note, wholly in the handwriting of Lavoisier. This note, probably written as early as 1792, appeared to be the programme of one or more prizes which it was proposed to found in the name of the Academy.

We have space for only a few reflections suggested by the interesting memoir of Dumas. Seventy-five years after the hand of Lavoisier traced those lines, the world gains from them new evidence of the genius of this great investigator. This memoir testifies also the great modesty of Lavoisier. To it he committed in a way calculated to conceal his personality, absorbed as it was by that of the Academy, truths which were revealed only a half century after his death and as the result of studies which employed twenty years of the lives of those who like Liebig and others, thought they had discovered them for the first time.

The following is the note of Lavoisier:—

"Vegetables obtain the materials necessary for their organization from the surrounding air, from water, and in general from the mineral kingdom. Animals feed either upon vegetables or upon other animals, which have been themselves nourished by vegetables, so that the materials of which they are formed are in the last resort drawn from the air or from the mineral kingdom.

"Finally, fermentation, putrefaction and combustion are perpetually returning to the atmospheric air and to the mineral kingdom the substances which vegetables and animals have borrowed from it.

"By what processes does nature keep in motion this wonderful circulation between the two kingdoms? How does she form substances combustible, fermentible and putrescible with combinations which did not previously possess these properties? These are impenetrable mysteries. We thus dimly perceive that while combustion and putrefaction are the means which nature employs for returning to the mineral kingdom the materials which she has drawn from it to form vegetables and animals, vegetation and animalization must be operations the reverse of combustion and putrefaction.

"It is therefore to animalization, or the nutrition of animals, that the Academy invites the attention of savans of all nations. The Academy does not conceal the fact, that the problem which it proposes for solution embraces an immense extent, that it supposes an analytical knowledge of those substances which serve as nutriment to animals, the alterations which they undergo successively in the canal which receives them by the mixture, 1st, with the saliva, 2d, by mixture with the gastric juice, 3d, by mixture with the bile, which supposes even to a certain extent the analytical knowledge of these different fluids and humors. It supposes also a knowledge of the gases which are disengaged during the process of digestion, the manner in which digestion furnishes to the blood that which is thrown off from it by respiration. In fine, as animals in a state of health and when they have attained their full growth recover from the losses suffered each day and regain the same weight they had the previous night, consequently the loss and gain are equal, the loss being compensated by the food which the animals consume daily.

"The Academy in thus presenting the whole scope of a great theme does not suppose that any one who undertakes will be able to resolve it completely. But the Academy permits each one to handle the problem in such a manner as he thinks convenient and the prize will be proclaimed in favor of him who shall resolve any portion of the subject. It hopes moreover to be able to recompense by its acknowledgements, those who undertake the investigation but do not obtain the prize.

"The Academy therefore intends to repropose from time to time the different portions of the same subject which have not been previously treated."

Dumas concludes his memoir as follows: "If it is asked what were the ideas and opinions of Lavoisier in regard to questions in organic chemistry, we must reply,

1st. He had discovered the process which serves to make all organic analyses, viz, their combustion by oxygen.

2d. That he had learned also, as his laboratory records show, that instead of burning them in oxygen gas he was able to burn them by means of metallic oxyds, and instead of measuring the resulting carbonic acid gas, he had the means of weighing it after having absorbed it in two successive flasks of liquid potassa.

3d. That he believed the bodies belonging to organic chemistry ought to be considered as the oxyds or acids of compound radicals.

4th. Lastly, that he understood the principal characteristics which distinguish the life of animals from that of plants; the character which pertains to each of the two organic kingdoms in the equilibrium of the forces of life; the part also which mineral matter serves in organic nature, combustion, and generally all which relates to its reduction."

The second unpublished document in this volume is by Leblanc, the inventor of the process for preparing soda from sea salt. This document relates to the mode of action of nitrogenous substances employed as fertilizers.

The tunnel of Mount Cenis.—We have frequently noticed this gigantic enterprise* the object of which is to pierce the Alps and to unite Italy and France by means of a grand tunnel passing under the *Col de Fréjus* to the southwest of Mount Cenis and where it had a height of 2949 metres (9675 feet.) We have seen that the subterranean passage terminates in Italy near the village of Bardonnèche and commences in Modane a small village in Savoy (in France). It debouches at a hundred metres above the road which leads to Turin across Mount Cenis. The subterranean way is 1338 metres (4390 feet) above the level of the sea. It has above it 1610·73 metres of calcareous rock. By reason of this enormous superincumbent mass, it is not possible to open ventilating shafts. The road will be subterranean through its whole extent and have no communication with the free air except at its two extremities. The length of the tunnel will be 12250 metres (40,191 feet or a little more than 7½ miles).

As yet the work of piercing the tunnel has advanced very slowly: on the side of Bardonnèche it has not advanced beyond 600 metres, and on the side of Modane where the rock is harder it proceeds with a little less rapidity and it has penetrated only 500 metres into the interior of the mountain. The works do not advance very rapidly and will require considerable time for their completion in spite of the labor incessantly put forth to perfect the means of excavation.

A problem more difficult to be resolved arises from the necessity of renewing the air in the interior of the tunnel, on account of the resistance which the compressed air meets with in the ventilating tubes. These tubes are 20 centimetres (about 8 inches) in diameter. The friction which they exert increasing in proportion to the distance passed over will be very great, perhaps not admitting of a solution. The question will be to give motion to machinery in the tunnel by means of air compressed in reservoirs with walls sufficiently strong as was proposed in 1858 in this Journal† as a motor suitable for working in long tunnels.

Obituary.—*Cordier* (Pierre, Louis, Antoine) died the 30 of March last at the age of 84 years. Born at Abbeville March 31st, 1777, in 1793 he entered the school of public works and passed in 1795 into the Corps des Mines. He joined the expedition to Egypt as a member of the Scientific Commission. He there executed many topographical labors, especially excavating the ruins of Sané (the ancient Tanis). On his return to Europe having been shipwrecked upon the coast of Calabria he profited by the misfortune by studying the mineralogy and geology of that province of Italy. Returning to France he resumed in 1808 his position of Engineer. Always at his studies, he continued, with the advice of Haüy, his researches in mineralogy and geology which were commenced under Dolomieu. In 1804 he was at the Canary islands where he studied the action of volcanoes.

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* This Journal [2], xxv, p. 96.

† Ibid. [2], xxvi, p, 98.

Taking for the object of his studies a science which was then in its infancy, Cordier became without much effort one of the founders of the science of geology. His investigations, the friendship of Haüy and the protection of the mineralogist Ramond, secured him admission to the Academie des Sciences where he took the place of Haüy in 1822.

Nominated general inspector of mines in 1831 he thenceforward performed the administrative duties of the office.

He was not without honors and dignity under the dynasty of Louis Philippe, and in the meanwhile he preserved all his independence and was never the courtier of power.

In the midst of his administrative functions Cordier still performed his duties as Professor of geology at the Jardin des Plantes, where he continued to the last a course in which geology was taught as it had been in 1825. Cordier had neglected paleontology; he was the oldest engineer of mines as he was the representive of the old school of geology, and he had little favor for the new theories and doctrines which had transformed science around him.

Acclimation.—Sheep.—The Zoological Society of Acclimation continue their labors with success, and find themselves in correspondence with all parts of the world. At the garden of acclimation which was founded in the Bois de Boulogne the Society has established weekly consultations upon different questions relating to acclimation and domestication; it has founded prizes many of which have been awarded this year to recompense real success. A number of these have been given for the propagation of sheep called *Graux de Mauchamp*, originated but a few years since and now celebrated for its beautiful and silken fleece. This race of sheep was originated about 1850 by a distinguished cultivator M. Graux: a lamb having been born at Mauchamp with a fleece of a remarkable silken quality he undertook to obtain a new race of sheep and by superior intelligence he was successful.

At present the silky wool called Graux de Mauchamp has taken an important place in French industry, and even in 1855 it had become celebrated. These results have been obtained by Davin a woolen manufacturer. Since then this manufacturer has introduced to commerce and industry the fleeces of *lamas*, and *alpacas*, the fine hair of the *camel* and of the Angora goat introduced into France and Algeria by the Society of Acclimation. The flesh of the Angora goat also constitutes an excellent article of food.

Acclimation of the Silkworm.—This question, in which the Society of Acclimation has been engaged since it was founded continues to be studied with great activity. The cold and rainy season of 1860 was not favorable for attempts at propagating in the open air either the silkworm of the Ailanthus glandulosa, or of the Ricinus; the latter of which has been introduced from the Canary isles. The difficulty in our country, is to delay the hatching of the cocoons until the Ricinus puts forth leaves.

M. Guèrin-Mèneville has been engaged in this enterprise and has already met with some success. This desirable result, of having a crop only in summer, has been realized with the silkworm of the mulberry tree, why has it not been obtained with that of the Ricinus?

Acclimation of the Ostrich.—For a long time the possibility of acclimating the ostrich has been doubted, the giant of birds, it has been said, like the elephant the giant of quadrupeds, refuses to permit its posterity to be enslaved. This is an error. In Algeria in the garden of acclimation the ostrich is fruitful and rears its young as well as at Florence in the zoological garden of Prince Demidoff and also in the royal park of acclimation in Spain. It remains to be known whether individuals born and reared in captivity can continue their species. If the Society of Acclimation undertakes the domestication of the ostrich it is because this bird furnishes a great trade, 1st, in plumes; 2d, in eggs, one of which is worth twenty-one hen's eggs; 3d, the flesh is very excellent. The society seeks to introduce in France and Algeria the *Nandon* (Nontornis) a small species of ostrich which lives in South America (Patagonia), the *Dromie casoar* of New Holland, which like the ostrich is a true bird of prey and the plumage of which is an article of commerce.

Acclimation of the Cotton-Plant.—We have recently mentioned the successful efforts made to popularize the culture of cotton in Algeria. Recently a new impetus has been given to this culture which in 1858 reached 1000 hectares and in 1859, 1,717. Laborers were wanting to give activity to this desideratum of commerce, but they are about to be supplied by the introduction of Chinese coolies. England is making great efforts for the culture of cotton in India, in Egypt, at Port Natal and in China. The interior of Africa where cotton grows spontaneously has recently attracted attention and the 750 bales of very fine Georgia cotton which Algeria furnished in 1858 have now (April 12, 1861) been excelled.

Bibliography.—The following works have been published by Hachette, *rue* Pierre-Sarrazin, Paris:

Leçons de Chimie professées en 1860, 1 vol. 8vo.—We have spoken above of this work in connection with the works of Lavoisier.

These lectures are upon subjects in which the several authors have made investigations or discoveries, viz:

Pasteur, upon molecular dissimilarity.

Cahours, on the history of organic radicals.

Wurtz, upon the glycols.

Berthelot, on synthesis in organic chemistry.

H. St. Claire Deville, on numerical laws in chemistry and the variation of their constants.

Barral, on the influence exercised by the atmosphere upon vegetation.

Dumas, historic memoirs concerning Lavoisier and Leblanc.

Payen.—*Précis de Chimie industrielle*, 4th edition. 3 vols. 8vo. with plates.—This fourth edition comprises a great number of improvements introduced in the chemical arts. The work is divided into two parts, viz. 1st, Chemical arts concerned with mineral substances. 2d, Industrial arts which deal with organic substances. The most important progress noticed in this work is in relation to *sulphur, oil, caoutchouc, glass,* *preservation of wood, sugar, distillation, gelatine, paper, illuminating gas, heating by gas, the bituminous cements*, &c.

Péclet.—Nouveaux documents relatifs au chauffage, 1 vol. 4to. with plates.—This volume which follows the great work of Péclet entitled "*Traite de la Chaleur*," relates especially to the heating and ventilation of public establishments, as hospitals and prisons. We find here interesting details in regard to the heating of the Hall of Sciences of the Institute and of the celebrated glassworks of Baccarat. There are also the usual formulas in relation to cooling and the transmission of heat through different bodies.

Payen.—Des Substances Alimentaires.—A small 12mo. treating of the methods of improving alimentary substances, of preserving them, and also of detecting their adulteration. This is a synopsis of the course of public lectures by Payen at the Conservatoire des Arts et Métiers, delivered by request of the government.

Heuzé.—Les Plantes Industrielles. 2 vols. 8vo.—This work follows a treatise on fertilizers by the same author, which we have already mentioned. The new work treats of the culture of plants which furnish the food of commerce and such as are used in the arts. Tome I treats of *oleaginous* plants, *tinctorial* plants, *saliferous* plants, and those used as *condiments*, &c. Tome II is devoted to *textile, narcotic, aromatic* and *medicinal* plants: and plants which produce sugar and alcohol.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

PHYSICS.

(1.) *On the propagation of heat in gases.*—MAGNUS has communicated to the Royal Academy at Berlin, a memoir on the propagation of heat in gaseous media, the principal results of which are as follows:

1. The temperature which a thermometer finally assumes in a space which is warmed from above is different when this space is filled with different gases.

2. This temperature is higher in hydrogen than in any other gas.

3. The temperature is also higher in this gas than in vacuo, and the greater the density of the gas, the higher is the temperature.

4. Hydrogen therefore conducts heat like the metals.

5. In all other gases, the temperature which the thermometer finally assumes, is lower than in vacuo, and the more dense the gas employed, the lower is the temperature.

6. We must not however conclude from this that gases do not conduct heat, but only that they do this to so small an extent that the action of the conduction is counteracted by the resistance which they oppose to the passage of heat.

7. The remarkable conducting power of hydrogen is shown not only when this is freely moveable but also when it is contained between pieces of eiderdown or any other substance of a loose texture which prevents its motion.

8. All gases, hydrogen included, offer resistance to the passage of rays of heat, and the more extended they are, the greater is this resistance.

9. Of all gases, atmospheric air or its constituents conduct heat most perfectly.

10. The passage of heat is different according to the source from which it comes. The rays emitted by boiling water exhibit the greatest differences in their passage through different gases.

11. Of all colorless gases, ammonia transmits the least heat; after this olefiant gas.

12. By the application of a tube, the action of the rays of heat can be increased like that of rays of light.

13. The character of the walls of the containing vessel changes the proportion in which rays of heat pass through the gases contained in the tube.

14. The character of the walls also changes the proportion in which the rays pass through different gases.

15. From this, it follows, that rays reflected from different surfaces are transmitted by gases, with different degrees of facility.

16. Hydrogen always transmits rays from different sources of heat, less easily than atmospheric air.

17. The marked increase in temperature which a thermometer placed in hydrogen undergoes when the gas is heated from above, does not therefore depend upon a greater capacity of transmission but only on a greater conducting power.

18. The greater conducting power of hydrogen for heat presents a new argument in favor of the analogy of this substance to the metals.

19. Hydrogen also conducts electricity better than the other gases.—*Pogg. Ann.*, cxii, 351. W. G.

2. *On a new Unit of Electrical Resistance.*—MATTIESSEN proposes to employ as a unit of resistance in electrical measurements, the resistance of a wire composed of two parts by weight of gold and one part of silver, having a length of one meter and a thickness of one millimeter. The author shows that this alloy conducts electricity with almost equal facility at different temperatures between 0° and 100°, that small quantities of impurities do not sensibly affect its conducting power, and that the annealing of the metal is also without sensible influence. The memoir contains empirical formulas by which the small differences in conducting power occasioned by variations in temperature may be taken into account. The standard wire should be varnished to protect it from the action of mercury.—*Pogg. Ann.*, cxii, 353. W. G.

Note.—It remains to be seen if the proposed alloy retains its specific conducting power after it has been frequently used. It is well known that wires of copper undergo great changes in this respect. W. G.

3. *On Spectral Observations.*—MOUSSON has described a simple arrangement of a prism for exhibiting the fixed lines in spectra from different sources; the author terms this apparatus a spectroscope. A description of the apparatus is prefaced by a mathematical investigation of the conditions which are necessary for the production of a perfectly distinct spectrum, for which however we must refer to the original paper. The apparatus itself consists essentially of a tube blackened internally, and having at one extremity a plate of metal, with an adjustable slit for the admission of light. The prism is placed at the other extremity of the

tube, so that the eye of the observer may be brought close to its second refracting surface. The tube is attached to an appropriate stand, so that it may be conveniently directed to the light to be examined, and the eye of the observer is protected from extraneous light by a small screen of metal attached to the tube. The edges of the slit must be ground perfectly true.

This apparatus does not require a darkened chamber or delicate and difficult adjustments. It answers equally well for observations on the solar spectrum, on the absorption lines of liquids and gases, on the electric spectra of Masson and Plücker, and on the chemical lines of Kirchhoff and Bunsen.—*Pogg. Ann.*, cxii, 428. W. G.

Note.—This apparatus is constructed in New York by Mr. Charles Sacher, under my direction, the prism being supplied by Mr. Henry Fitz, the well-known optician. The price of the instrument complete with an equilateral flint glass prism, is $25. W. G.

4. *On the Absorption and Radiation of Heat by Gases and Vapors, and on the Physical connection of Radiation, Absorption and Conduction.*—TYNDALL has communicated an important and interesting paper on the absorption and radiation of gases which is in some respects complementary to that of Magnus noticed above. The apparatus employed consisted of 1. a copper cube with one of its faces covered with lampblack and filled with water kept boiling. This forms the source of radiant heat; 2. of a brass tube 2·4 inches in diameter and divided into two compartments, *a* and *b*. The portion *a* is destined to receive the gases and vapors; it is closed at its two extremities by two transparent plates of rock salt and communicates with a good air pump: its length is four feet; B is the chamber between the tube *a* and the cube C. A vacuum is kept in this tube, so that the radiant heat traverses the vacuum, before entering the tube *a*. To prevent the transmission of heat by metallic conduction from the cube C, to the tube *a*, the chamber *b* is partly surrounded with an annular space in which cold water circulates. 3. Of a thermo-electric pile furnished with two conical reflectors, and connected with a galvanometer; one face of the pile receives the rays which have traversed the tube *a*. 4. Of a second cube C′ also filled with boiling water and the rays from which fall upon the second face of the pile. Between the cube C′ and the adjacent surface of the pile, a screen is placed, which may be moved backward and forward, so as to make the two sources of heat exactly neutralize each other. A vacuum is then made in the tube *a*, and the chamber *b*, and the needle of the galvanometer is brought exactly to 0 by means of the screen. The gas or vapor to be experimented upon is then introduced into the tube *a*; if it possess a sensible power of absorption, it will destroy the equilibrium previously existing. The deviation of the galvanometer properly reduced gives the measure of the absorption.

In this manner the author experimented with eight gases and 13 vapors, and also with atmospheric air. Oxygen, nitrogen, hydrogen and atmospheric air absorb respectively about 0·3 per cent of the rays of heat; this was the feeblest action observed. The strongest action is that of olefiant gas, which under a tension of one atmosphere absorbs 81 per cent of the calorific rays. Between these two extremes must be placed oxyd of carbon, carbonic acid, protoxyd of nitrogen, and sulphydric acid.

Below a certain tension which varies with different gasses, the quantity of heat absorbed is exactly proportioned to the density of the gas. Above this tension, the rays on which the principal power of absorption is exerted, are gradually extinguished so that each increase of density produces a less effect. In the case of olefiant gas for instance, by taking a volume of $\frac{1}{50}$ of a cubic inch as unity in a series of fifteen of these volumes, an absorption was obtained exactly proportioned to the quantity of gas; then the relations of the successive absorptions gradually approach an equality.

In the case of vapors, the most energetic action is that of sulphuric ether; the least energetic is that of bi-sulphid of carbon. By comparing small volumes and equal tensions, the absorption of the vapor of sulphuric ether is ten times greater than that of olefiant gas, and 10,000 times greater than that of hydrogen, oxygen, etc. In a fine day in November, the aqueous vapor in the atmosphere produced 15 times the absorption of the air itself. This great absorbing power is exerted upon rays coming from a source of low temperature, whence we must conclude that the aqueous vapor in the atmosphere must powerfully intercept the rays which tend to pass from the earth into the planetary spaces. Variations in the quantity of vapor in the atmosphere would therefore necessarily produce corresponding variations in climate. Oxygen obtained by the electrolysis of water has a power of absorption four times greater than the same substance which has been passed through iodid of potassium. This increase is due to the presence of ozone.

The author studied the radiation of gases by making them pass over a sphere of heated metal from which they rose in a column in front of the thermo-electric pile. In this manner it was found that the order of radiation is exactly the same as that of absorption.

This reciprocity of absorption and radiation appears to the author a simple mechanical consequence of the theory of an ether; but why has one molecule so great and another so feeble a power of producing or arresting calorific rays? The author suggests that the explanation is to be found in the fact that the gaseous elements examined all exhibit radiations and absorptions which are excessively feeble when compared with those of compound gases. In the first case, the action is produced by oscillating simple atoms—in the second place, by oscillating systems of atoms. Thus oxygen and hydrogen, which taken separately or mechanically united produce an effect which is scarcely sensible, when they are chemically united to form oscillating systems of aqueous vapor, are able to produce a great effect.

In like manner, nitrogen and hydrogen which produce little effect when they are separated, exert an enormous action when they are combined to form ammonia. The same relation exists between the absorbing and radiating powers of other simple gases and their compounds.—*Comptes Rendus*, T. lii, p. 364. W. G.

5. *A Note on the power of Polarization of American Oil of Turpentine;* by Dr. F. Mahla. (In a letter to the Editor.)—It has been already mentioned by Guibourt and Bouchardat, that the American oil of turpentine possesses a power of rotation of 18°·6 to the right.

Numerous experiments with commercial spirits of turpentine enable me to state, that its power of rotation is far from being constant. I have examined specimens, which had a power of rotation of 14° (28° in my instrument with a tube of 200 millimetres) while others turned it 20° to the right. Most of the specimens possessed a power of rotation of 19°·5.

Oil of turpentine of 19°·5 subjected to distillation together with water, yielded two oils of different rotating power. The first distillate rotated 22°·5,—the last portion 16°·38.

The rotating power of neither of these two portions was changed by redistillation.

The boiling point of the first portion was a few degrees lower than that of the second portion. It commenced boiling at 295° F., while the thermometer was constantly rising, until it reached 312°. The second portion did not boil before the thermometer had reached 309° F. The final boiling point of the latter did not exceed 315° F.

These data seem to indicate that the American oil of turpentine consists of two or more different hydro-carbons, which are probably polymeric to each other.

Chicago, Jan. 12, 1861.

CHEMISTRY.—

6. *On Salts of Chlorine and other Electro-negative Elements.*—SCHÜTZENBERGER has succeeded—according to his own statement—in preparing acetates of chlorine, iodine and other electro-negative radicals. When acetate of soda is mixed with chlorid of iodine the ensuing reaction is represented by the equation

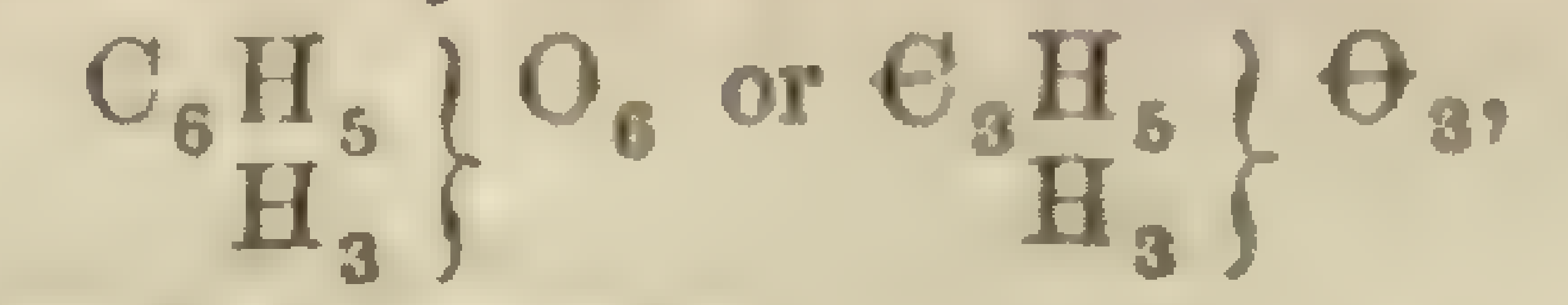
$$C_4H_3NaO_4+Cl\,I=C_4H_3O_4I+NaCl.$$

The new substance $C_4H_3O_4I$ is solid, white, and crystalline; it is isomeric with iodacetic acid $C_2(H_2I)O_4H$ but differs from it entirely in properties. Anhydrous acetic and hypochlorous acids unite at a low temperature and form acetate of chlorine,

$$C_4H_3O_3+ClO=C_4H_3O_4Cl.$$

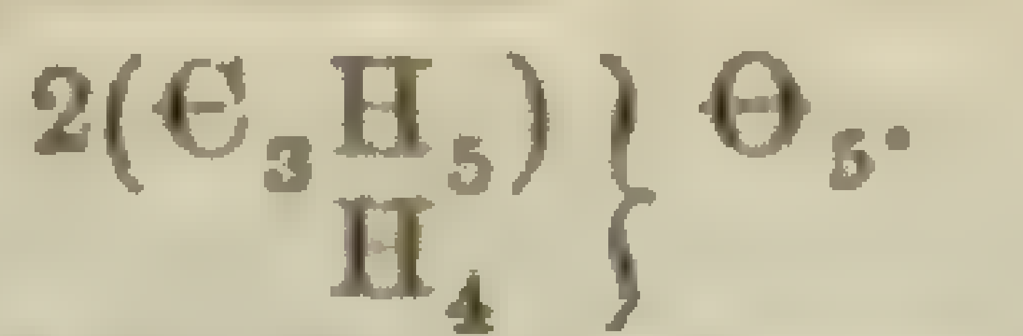
This compound is a colorless liquid: it is decomposed by water into acetic and hypochlorous acids; it explodes violently near 100° giving chlorine, oxygen and anhydrous acetic acid. Even at ordinary temperatures it is gradually decomposed. An acetate of cyanogen appears to be formed by heating a mixture of equal equivalents of acetate of silver and iodid of cyanogen. Anhydrous sulphuric acid absorbs anhydrous hypochlorous acid, giving a very stable dark red liquid which the author believes to be a sulphate of chlorine SO_4Cl or $S_2O_8Cl_2$. These results require confirmation but if reliable are of great interest and importance.
—*Comptes Rendus*, lii, 135. W. G.

7. *On Polyglyceric Alcohols and Anhydrids.*—LOURENÇO has found that glycerine like glycol is capable of forming polyatomic molecules by what may be regarded as successive condensations. Thus ordinary glycerine being represented by the formula



$$\left.\begin{matrix}C_6H_5\\H_3\end{matrix}\right\}O_6 \text{ or } \left.\begin{matrix}\text{Є}_3H_5\\H_3\end{matrix}\right\}\Theta_3,$$

diglycerine alcohol has the formula



$$\left.\begin{matrix}2(\text{Є}_3H_5)\\H_4\end{matrix}\right\}\Theta_5.$$

It is a colorless thick liquid boiling at 220°–230° in vacuo. The author compares this body to pyrophosphoric, phosphoglyceric, and citroglyceric acids, the formulas being

$$\left.\begin{array}{r}\text{Є}_2\text{H}_5\\ \text{Є}_3\text{H}_5\\ \text{H}_4\end{array}\right\}\Theta_5,\quad \left.\begin{array}{r}\text{P}\Theta\\ \text{P}\Theta\\ \text{H}_4\end{array}\right\}\Theta_5,\quad \left.\begin{array}{r}\text{P}\Theta\\ \text{Є}_3\text{H}_5\\ \text{H}_4\end{array}\right\}\Theta_5,\quad \left.\begin{array}{r}\text{Є}_3\text{H}_5\\ \text{Є}_6\text{H}_5\Theta_4\\ \text{H}_4\end{array}\right\}\Theta_5.$$

A second compound boils at 275° to 285° under a pressure of 10^{mm} and is still more viscid.

Its formula is $\left.\begin{array}{r}\text{Є}_3\text{H}_5\\ \text{Є}_3\text{H}_5\\ \text{Є}_3\text{H}_5\\ \text{H}_5\end{array}\right\}\Theta_7$: it appears to lose water by repeated distillations and to give its first anhydrid $\left.\begin{array}{r}3(\text{Є}_3\text{H}_5)\\ \text{H}_3\end{array}\right\}\Theta_6$.

The author also obtained a colorless oily liquid having the formula

$$\text{Є}_6\text{H}_{12}\Theta_4=\left.\begin{array}{r}\text{Є}_3\text{H}_5\\ \text{Є}_3\text{H}_5\\ \text{H}_2\end{array}\right\}\Theta_4.$$

This compound is formed from pyroglycerine by the elimination of two equivalents of water. The author terms it meta-glycerine or pyroglycide and compares it with Maddrell's insoluble metaphosphate $\left.\begin{array}{r}\text{P}\Theta\\ \text{P}\Theta\\ \text{Na}_2\end{array}\right\}\Theta_4$.

Glycide (not yet obtained) would correspond to Graham's metaphosphate

$$\left.\begin{array}{r}\text{Є}_3\text{H}_5\\ \text{H}\end{array}\right\}\Theta_2 \text{ to } \left.\begin{array}{r}\text{P}\Theta\\ \text{Na}\end{array}\right\}\Theta_2,$$

while one of Fleitmann's and Henneberg's metaphosphates would correspond to a triglycerine anhydrid

$$\left.\begin{array}{r}3\text{P}\Theta\\ \text{Na}\end{array}\right\}\Theta_6 \text{ to } \left.\begin{array}{r}3\text{Є}_3\text{H}_5\\ \text{H}_3\end{array}\right\}\Theta_6.$$

These parallels are very interesting and suggestive.—*Comptes Rendus*, lii, 359. W. G.

8. *On Ozone, Nitrous Acid and Nitrogen;* by T. Sterry Hunt, F.R.S. (Extract of a letter to one of the Editors, Jan. 29, 1860).—The formation of a nitrite when moist air is ozonized by means of the electric spark (the old experiment of Cavendish,) or by phosphorus was shown by Rivier and de Fellenberg, who concluded that the reactions ascribed by Schönbein to ozone were due to traces of nitrous acid. The subsequent experiments of Marignac and Andrews have however established that ozone is really a modification of oxygen, which Honzeau has shown to be identical with the so-called nascent oxygen, which is evolved, together with ordinary oxygen, when peroxyd of barium is decomposed by sulphuric acid at ordinary temperatures. The spontaneous decomposition of a solution of permanganic acid also evolves a similar product having the characters of ozone.

Believing that the nitrous acid in the above experiments is not an accidental product of electric or catalytic action, but dependent upon the formation of active or nascent oxygen, I caused a current of air to pass through a solution of permanganate of potash mixed with sulphuric acid. The air, which had thus acquired the odor and other reactions of ozone was then passed through a solution of potash; by which process it lost its peculiar properties while the potash solution was found to contain a salt having the reactions of a nitrite.

As I suggested in this Journal in 1848, I conceive gaseous nitrogen to be the anhydrid amid or nitryl of nitrous acid; which in contact with water might under certain circumstances generate nitrous acid and ammonia. From the instability of the compound of these two bodies, however, it becomes necessary to decompose one at the instant of its formation in order to isolate the other. Certain reducing agents which convert nitrous acid into ammonia may thus transform nitrogen (NN) into $2NH_3$. In this way I explain the action of nascent hydrogen in forming ammonia with atmospheric nitrogen in presence of oxydizing metals and alkalies. (Zinc in presence of a heated solution of potash readily reduces nitrates and nitrites with the evolution of ammonia.)

Now an agent which instead of attacking the nitrous acid would destroy the newly formed ammonia would permit us to isolate the nitrous acid. Houzeau has shown that nascent oxygen is such an agent, at once oxydizing ammonia with formation of nitrate (nitrite?) of ammonia, and thus when ozone (nascent oxygen) is brought in contact with moist air both of the atoms of nitrogen in the nitryl (NN) appear in the oxydized state.

From this view it follows that the odor and most of the reactions ascribed to ozone are due to nitrous acid which is liberated by the decomposition of atmospheric nitrogen in presence of water and nascent oxygen. We have thus a key to a new theory of nitrification and an explanation of the experiments of Cloez on the slow formation of nitrite by the action of air exempt from ammonia upon porous bodies moistened with alkaline solutions. I hope soon to send you the results of my farther inquiries in this matter.

New Books.

9. *Die Lehre vom Galvanismus und Electro-magnetismus, von* Gustav Wiedemann. Braunschweig, 1861. 8°.—The first volume of an elaborate treatise on galvanism and the kindred subjects has just appeared under the above title. The work is in all respects admirably executed and is most complete and thorough. The text is copiously illustrated by excellent wood engravings, each special subject being treated from a mathematical as well as from an experimental point of view. A serious defect is the want of a complete and detailed table of contents or index, which would render the work much more convenient for the purposes of reference. The work is much too elaborate and extensive for a text book, but it presents so complete a view of the present state of the science that it will prove most valuable to the advanced student. It is to be hoped that the second volume will speedily appear. W. G.

10. *Der Electro-magnetismus von* Julius Dub. Berlin, 1861. 8°.—This treatise is devoted almost exclusively to the subject of the development of magnetism in soft iron; though the first sections give the theory of the galvanometer in its different forms. The subject of electro-dynamics is not included. Though the author restricts himself to a comparatively narrow field, the work is extensive and thorough, and forms a most valuable contribution to our scientific literature. Prof. Riess' admirable

treatise on Statical Electricity with the works of Wiedemann, DuBois—Raymond and Dub form an almost complete library of reference for the subject of electricity. A complete general treatise on magnetism is still wanting. W. G.

11. *Les Electro-Aimants et L'Adherence Magnetique par* M. J. NICKLÈS, Paris, 1860. 8vo.—This book is devoted chiefly to a recital of the Author's experiments on various forms of Electro-magnets made during an investigation to test the adaptation of magnetic adhesion for arresting the motion of railway trains. Most of the experimental data have already appeared in this Journal and the Author's proposed nomenclature was given in vol. xxx, p. 413. S.

12. *Die Fluorescens des Lichtes-umgetragen von* J. PISKO. Wien, 1861. 8°.—This pamphlet is a rather popular compilation from the different memoirs which have appeared upon the interesting subject of which it treats. It will prove convenient for reference, especially for the purposes of the lecture room. W. G.

13. *Einleitung in das Studium der Organischen Chemie, von* J. SCHIEL. Erlangen, 1860. 8°.—This excellent treatise followed close upon the author's work on Organic Analysis,* and will be well received as a clear and careful exposition of the views of the new school in chemistry. We are especially pleased with the attention which the author pays to the *physical* portion of the subject. The work includes sections upon crystallography, expansion, fusing and boiling points, tension of vapors, latent and specific heats, densities, specific volumes, evolution of heat in combustion, optical constants, and laws of absorption and cohesion. The chapter on the classification of organic bodies by series is especially original and valuable and the work well deserves translation. W. G.

14. *Allgemeine Encyclopädie der Physik.—Herausgegeben von* GUSTAV KARSTEN. Leipsig. 8°.—The eighth Lieferung of this work has reached us and contains the last part of Schmid's elaborate treatise on Meteorology together with a continuation of Helmholtz's admirable paper on Physiological Optics. The treatise on Meteorology forms the 21st and last vol. of the entire work, the publishers wisely issuing the several treatises as fast as they are prepared without reference to the order of the volumes. The list of contributors embraces many of the first scientists of Germany and the work promises to be a worthy successor to the Physikalisches Wörterbuch of Gehler, now far behind the present state of science, though indispensable as a work of reference. The new Encyclopedia forms properly a collection of separate treatises, each complete and available in itself. The programme embraces 1. Introduction to physics. 2. Crystallography. 3. General theory of forces. 4. Attractions. 5. Applied mechanics. 6. Theory of waves and acoustics. 7 and 8. Pure optics. 9. Physiological optics. 10. Chemical action of light. 11. Applied optics. 12. Action of heat. 13. Theory of Heat. 14. Applied Thermics. 15. Magnetism. 16. Magnetism of the Earth. 17. Frictional Electricity. 18. Galvanism. 19. Electro-magnetism. 20. Applied Electricity. 21. Meteorology. We trust that the work will be pushed forward with an energy commensurate with the magnitude of the undertaking. W. G.

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* See notice in this Journal, xxx, 155.

15. CHEMICAL GEOLOGY:—*Experiments on the deportment of Carbonate of Lime at a high temperature, both with fluxes and alone;* by G. ROSE. —In a recent paper on the heteromorphous states of carbonate of lime, presented to the Berlin Academy of Sciences, Rose gives the following experimental results: I. On heating a mixture in atomic proportions of the carbonates of soda and potash in a platinum crucible until in perfect fusion, then adding small quantities of chlorid of calcium, the latter will be completely dissolved without effervescence. If the fused mass is allowed to cool and a portion of this is placed in water at the ordinary temperature, it will gradually pass into solution leaving a pulverulent residue of carbonate of lime. An examination of this residue shows it to consist of an aggregate of very small globules; after 24 hours—sometimes in less time—these increase in size and are converted either into beautifully crystallized single rhombohedrons, or rhombohedral groups of *calcite.*

II. If another portion of the mass is thrown into boiling water, boiled for some time, and the residue examined under the microscope, it will be found to consist of small prisms of *aragonite*, with occasional rhombohedral crystals of calcite, but none of the above mentioned globules; if the residue be allowed to remain under water, the prisms after a time are converted into minute rhombohedrons of calcite. These phenomena are then identical with those obtained when a mixture of the solutions of carbonate of soda and chlorid of calcium is made, as long since described by the author in his first article on this subject, see *Pogg. Ann.*, (1837), xlii, 354.

III. Instead of adding chlorid of calcium to the fused carbonates, powdered calcite, or rhombohedral fragments of calcite, chalk or aragonite may be substituted; these dissolve completely, without effervescence in the flux and give the same results as above mentioned when the fused mass is treated with hot or cold water.

IV. Oxalate of lime at a low red heat after losing the water it contains is converted into carbonate of lime with evolution of carbonic oxyd; under the microscope the product appears to consist of minute amorphous globules* similar to those obtained above, and these remain unchanged when placed in water, even when boiled the globules still retain their amorphous form—they are not converted into calcite.

From the experiments thus far described it will be seen that rhombohedral carbonate of lime is never a direct product. But according to the well known experiments of Sir James Hall made in 1804, this has been directly produced when chalk and compact limestone were exposed to a high temperature under great pressure. The author assisted by Mr. Warren Siemens, has repeated Hall's experiment. A gun barrel was charged with dry elutriated chalk, this last was rammed into a compact mass, and then the gun-barrel was hermetrically sealed at both ends, and exposed to the heat of one of Mr. Siemens' gas furnaces. During the experiment the barrel sprung, and in the crack there appeared a faint blue flame, evidently of carbonic oxyd, the gun-barrel was then removed from the furnace. Upon opening the barrel, the chalk was found converted into a compact, light bluish-white coherent mass, slightly lustrous on the fracture, and with cracks running through the whole. The surface was covered with a snow-white, earthy, well defined crust and the cracks were lined with white earthy particles, these, as well as the crust, were composed of caustic lime. The compact mass, however, on examination proved to be unchanged in chemical properties, and its physical properties, though seemingly changed, when examined under the microscope showed the same small globules, and identically the same properties as the unignited amorphous chalk. Although somewhat more coherent the chalk was not materially altered and in no wise converted into crystalline calcite.

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* Oxalate of lime is amorphous, and when converted by heat into carbonate, this character still remains.

The experiment repeated with fragments of rhombohedral calc-spar was also interrupted by the rupture of the gun-barrel. The smaller fragments were converted into caustic lime without change of form, the larger pieces were only superficially altered, notwithstanding the great heat, the inner mass was unchanged and the lime of the surface-crust sharply defined. The same result was obtained by the author under different conditions: Mitscherlich presented him with a mass of limestone from Rüdersdorf which, on account of its size had passed through the lime-kiln without being completely burned. It contained a kernel of unburned lime and, notwithstanding the great heat through which this had passed it proved on examination to have the same characters as the compact limestone which had not been exposed to the heat of the kiln.

It appears therefore, from these experiments, that chalk or compact limestone cannot be converted into crystalline limestone (or calc-spar) by exposure to a high temperature in closed vessels, and as a general fact that rhombohedral carbonate of lime is not formed in the dry way. The author further observes, that on comparing accurately the description of Hall's* experiments and those afterwards made by Bucholz†, that probably they obtained results similar to those just described, and that the slightly coherent but otherwise unaltered mass, was erroneously considered to be crystalline marble.

Notwithstanding the frequency with which this experiment of Hall's has been quoted, and the use that has been made of it not only in explaining geological phenomena, but in serving as the foundation of whole theories, it was never repeated‡ or confirmed and the experiments of the author show how hasty the conclusion was. It is not to be disputed that at the junction with granite and basalt, compact limestone and chalk are often converted into marble, as in Paradiesbacken near Drammen in Norway, and near Belfast, in Ireland, but these changes cannot be considered as due to heat alone, they were manifestly assisted by other agencies, a conclusion also arrived at by Bischof, although in a different manner.—*Pogg. Ann.*, cxi, 156. G. J. B.

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* *Gehlen: Neues allgem. Journ. d. Chem.*, v. 287.

† *Gehlen: Journ. f. Chem. u. Phys.*, i, 271.

‡ Bucholz made his observations incidentally in the production of caustic lime from chalk, which in the experiment had not been entirely burned.

TECHNICAL CHEMISTRY.

16. *On the occurrence of soluble compounds of Copper, Lead and Tin in newly-distilled Alcoholic Spirits.*—In an article discussing the question of the existence of poisonous bodies, especially strychnine, in the spirits produced from grain, which has been so frequently agitated by the public journals during the past few years, Dr. A. A. HAYES expresses his conviction that no good reason can be found for the statements affirming the existence of such substances, which have been made by the newspapers; and remarks that his opinion is founded not only upon an extensive series of analytical observations but is supported by the testimony of the parties most likely to know of such adulteration if it were really made, none of whom had ever met with a case at the places of manufacture which would lead even to an inference that any deleterious body had been purposely added to the distilled spirits.

But while dismissing this bugbear, Dr. Hayes calls attention to another source of danger which may be of importance. "Newly distilled spirits, of the most common kind, often contain *salts of copper, of lead, or tin* derived from the condensers, in which the vapors are reduced to a fluid form. The quantity of copper salt contained in the bulk usually taken at a draught, is sufficient to produce the minor effects of metallic poisoning; the cumulative character of these poisons may even lead to fatal consequences. With a knowledge of the fact now stated, instead of resting on a supposition of the existence of an organic poison in the spirit, which may have caused sickness, the physician should notice the symptoms of metallic poisoning in persons addicted to the habit of consuming newly-distilled spirits, and interpose his aid in preventing the fatal termination of vicious indulgence."

The author goes on to remark that since he first became aware of the frequent occurrence of these metallic salts in recently manufactured spirits, he has made investigations which "prove that as all spirits at one time were new, so with few exceptions—arising from peculiar rectifications—most spirits have been, or are, more or less contaminated by metallic compounds. Old or more matured spirits have generally lost every particle of the salts once held in solution. Changes in the organic solvent have caused the deposition of the metallic compound accompanied by the organic matter from obvious sources, and in such spirits the metallic oxyd is always found—if it has been present—in the dark colored matter which has been deposited at the bottom of a cask at rest. This dark deposit has the appearance of, has been mistaken for, charcoal, detached from the charcoal staves of the casks in which the spirits have been stored. Of this dark deposit every sample has, on examination, afforded abundance of copper, copper and tin, or copper and lead, even when taken from the finer qualities of foreign spirits. Observations have been made on the nature of this change from a soluble to an insoluble state. Samples of new spirits have been kept in glass vessels until the whole metallic salt has fallen in dark flocks, leaving the clear fluid free from any metallic compound and perefectly pure. It appears therefore that matured spirits lose their poisonous impregnation during the time necessary to adapt them for use as beverages, and that while the clear, transparent fluid contains no metallic impregnation, a turbid though ripened spirit may prove deleterious, through its *suspended* metallic compounds. In order to avoid the poisonous effects of these salts, perfectly well-ripened and clear spirits only should be used in the preparation of medicines, and when ordered as restoratives, no new or turbid alcoholic fluids should be allowed to enter the hospital or room of the patient."

Dr. Hayes explains the origin of these salts as being connected with the production of acids, as well as alcohol in the fermenting vats. "When the wort is subjected to heat in the still, acetic, butyric and other acids rise with the vapor of alcohol, and pass into the condenser, now most commonly made of copper, with masses of solder containing lead. At the instant of condensation, these acids exert a power of corrosion on the metals quite unsuspected, and the salts formed dissolve in the spirit. Where condensers of pure tin are used, no copper salt is formed, and a little tin salt takes its place. With the vapor of dilute alcohol some vesicular vapor of the wort is carried forward, and the dextrine which can be found in the spirit; another portion of soluble organic matter is abstracted from the wood of the cask, and this is often tannic acid. In the subsequent chemical changes these organic compounds unite with the salts, and fall in the form of a sub-granular dark matter seen in colorless spirits of all kinds. In detecting the metals held in *solution*, the extract obtained, after evaporating the spirit, must be destroyed, as usual in toxicological testing, and an acid solution of the oxyd obtained; or the extract may at once be mixed with carbonate of soda, and the metal reduced by the blowpipe flame. When the deposit is the subject of trial the metal or metals appear on fluxing with carbonate of soda, in the inner flame produced by the blowpipe, on charcoal.—*Boston Medical and Surgical Journal*, lxiii, 169.

17. *Local Decomposition in Lead Aqueduct Pipes.*—Mr. J. R. NICHOLS of Boston calls attention to a source of danger attending the use of leaden pipes used for the conveyance of drinking water which seems to have been hitherto disregarded. Even if it be admitted that the water which is supplied to the city of Boston from Lake Cochituate, like that of most New England ponds, be such that it may be safely used after having passed through lead pipe under ordinary circumstances, it would nevertheless be wrong to infer that this water can be employed with entire safety at all points of delivery, without first inquiring whether special conditions may not exist in some localities by which the character of the water may there be changed. Having observed several instances in which the inmates of a single house had suffered from lead disease induced by the use of aqueduct water, while the inhabitants of other parts of the city supplied with water from the same original source were unaffected, and having in one instance detected the presence of considerable quantities of lead in one of the cases first mentioned, while no reaction for lead could be obtained from a specimen of the same aqueduct water taken from another locality, the author proceeded to inquire into the cause which produces this lead impregnation in certain houses or districts while the general waters of a supply remain unaffected. He has noticed in the leaden pipes removed from cess-pools, sinks and wells, that the intensity of corrosive action had been in great measure confined to the sharpest bends and depressions in the pipe, while in some instances other portions remained intact. "I

have in my possession," he says, "a section of supply pipe, removed from the aqueduct of a neighboring city, in a portion of which corrosive action had proceeded so far as to cause leakage. The part thus acted upon was confined to an acute angle, and there is evidence to show that the plumber, in placing it in position, bent it in the wrong direction, thus creating the necessity for another turn in the opposite. This pipe had doubtless been subjected to two violent turns, which seriously impaired the homogeneity of the metal. An examination of lead pipe removed from buildings will certainly show that where there has been any perceptible amount of decomposition, it has been confined to the angles and depressions in its course. There are three causes or agencies which may perhaps be sufficient to produce these results:—1. The disturbance in the crystalline structure of the metal by bending, whereby its electrical condition is changed and voltaic action promoted, giving rise to chemical decomposition. [Together with the galvanic action which must be induced wherever connections or faucets of copper, or alloy, are fastened to the leaden pipes, or where a crack or fissure in the latter has been filled with solder. (See in this connection, an article by the Editor of the Boston Med. and Surg. Journ., xl, 125, 1849).—F. H. S.] 2. The presence of organic matter, such as fragments of leaves, and impurities pervading all pond waters, and which may be detained in angles and depressions of the pipes. Their presence, undoubtedly, promotes oxydation [?] and the protoxyd of lead will remain in solution, unless sufficient carbonic acid is furnished to change it. It is easy to conceive of conditions where this could not be the case. 3. Corrosions may be produced in lead pipes by the accidental presence of pieces of mortar. Where mortar is present, the lime would assist in oxydizing the metal, and also aid in the solution of the oxyd. Considerable portions of fresh mortar are frequently deposited in lead pipes, during the erection of buildings. When the family commence the use of the water, it holds the salts of lead in solution, and its presence may be detected for months. The process of oxydation, which is retarded or prevented altogether by the presence of neutral salts in water, could not be materially interfered with under the conditions considered. It is obvious, if these observations and conclusions are correct, that much care should be exercised in placing pipes in position in buildings. In those leading to the culinary department, angles and depressions should be avoided. Violent twists and turns should not be permitted, and during the erection of houses, the open ends of protruding pipes should be carefully closed. Assuming the general fact that lead pipes, conveying the waters of our New England ponds, become coated and protected by an insoluble lead salt, the question arises, how long before this protection is secured, or, how soon may a family commence the use of water passing through new pipes, with safety? In view of the manifest danger from local disturbances, the most sensible reply would be, *never.* A section of new lead pipe immersed in Cochituate water one hour, at a temperature of 65° F. gave a decided lead reaction with sulphydric acid. Removed and placed in six fresh portions of water one hour in each, the waters, when tested, gave similar results. The experiment continued during two weeks. Varying the time of immersion in fresh portions of water from one to ten hours the lead indications continued, although at last feeble. These results are sufficient to show that individuals or families should not commence the use of water flowing through new pipes, until considerable time has elapsed, and much water contact secured."—*Boston Medical and Surgical Journal*, lxiii, 149.

18. *Nature of the deposit which forms upon the copper employed in Reinsch's test for arsenic.*—LIPPERT has made a careful examination of the crust which forms upon bright metallic copper when this is placed in a solution of arsenic acidified with chlorhydric acid. This coating had been pretty generally mistaken for metallic arsenic until Fresenius (in his *Anleitung zur qualitativen Analyse*, 10*te Aufl.*, Braunschweig, 1860, p. 141,) called attention to the fact that it contained a large quantity of copper. From the experiments of Lippert it now appears that the crust in question contains only 32 per cent of arsenic, 68 per cent of its weight being copper. This composition having been nearly constant in several specimens which he analysed, Lippert maintains that the compound is a definite alloy $AsCu_5$. [Being evidently unaware of the experiments of Prof. Cooke, (see this Journal [2,] xx, 222) which have shown how little ground we have for believing in the existence of alloys of definite composition in the case at hand.—F. H. S.] When ignited, at the temperature of a combustion furnace, in a current of hydrogen, the compound lost only 7 per cent of its weight, an alloy of the composition $AsCu_6$ (same as that of the mineral Domeykite of F. Field, see this Journal [2,] xxv, 406.) being formed.

The delicacy of Reinsch's test is evidently directly referable to the large amount of copper which the characteristic coating contains, for a proportionally small quantity of arsenic is thus obtained in an enlarged and as it were more tangible, form. But on the other hand it is not easy to prove, in a simple manner, the presence of arsenic in this crust, for only a small portion of the arsenic can be volatilized in a current of hydrogen, and even if the alloy be first oxydized in a current of air and then reduced in a current of hydrogen the percentage of arsenic only falls from 32 to 20. By far the largest portion of the arsenic is therefore kept out of sight.

For the details of this interesting research and the author's discussion of the proposition of Reinsch and v. Kobell to estimate arsenic quantitatively by determining the amount of copper which dissolves while the arsenic is being precipitated we must refer to the original article.—*Journal für prakt. Chemie*, lxxxi, 168.

19. *Saponification by alkaline carbonates in the dry way.*—As an addition to the experiment of Pelouze (Ann Ch. et Phys. (3,) xlvii, 371) by which it was demonstrated several years since that soap could be prepared by heating together a mixture of fat and an anhydrous alkaline oxyd at a temperature of 250° (C.), SCHEURER-KESTNER now shows that the alkaline carbonates can be used instead of the oxyds, carbonic acid being disengaged while a true soap is formed. Thus, when a mixture of 100 parts of tallow and 22 to 25 parts of carbonate of soda is gradually heated, a lively reaction commences at about 260° (C.) [=500° (F.)], the mass swells up while an abundant evolution of gas occurs:—in order to prevent loss it is necessary to conduct the operation in a spacious retort and to moderate the heat as soon as the reaction has once begun; if in addi-

tion to this the retort be shaken from time to time in order to mix the ingredients, which have a tendency to separate, the disengagement of gas will proceed with great regularity. Towards the close of the operation, the temperature must be slightly increased in order to decompose the last traces of the fat which is more difficultly acted upon now that it is no longer in presence of so large an excess of the carbonate. At the end of several hours a yellowish, semifluid mass is obtained which becomes more consistent on cooling; it dissolves slowly in water, producing an opaline liquor resembling in all respects a solution of common soap.

When carbonate of lime is substituted for carbonate of soda, in the proportion of 18 to 20 parts of the lime-salt to 100 pts. of tallow, the reaction occurs more readily than when carbonate of soda is employed, a slightly yellowish mass, hard enough to be pulverized, being obtained. With the carbonates of baryta, strontia and magnesia, the same reaction occurs, while with carbonate of lead, the decomposition goes on very rapidly and is accompanied with a tumultous evolution of gas.

If the action of the heat is carefully regulated, oxyd of glyceryl is alone decomposed, the fatty acids remaining intact. By direct experiment 100 pts. of tallow when saponified with carbonate of soda afforded 94·8 grms. of fatty acid; while the same amount of tallow saponified by carbonate of lime yielded 95·6 pts. of fatty acid.

100 grms. of the lime soap treated with water gave no glycerine; but by means of ether a few centigrms. of a nonvolatile oily substance, lighter than water and insoluble in alkalies or mineral acids, were extracted. The gases disengaged consist of carbonic acid, about 75·30 pr. ct., light carburetted hydrogen 11·85 pr. ct., hydrogen 12·85 pr. ct., together with a little acroleine.—*Annales de Chimie et de Physique*, [3], lx, 216.

20. *Preparation of Hydrate of Baryta by means of Oxyd of Zinc.*—Instead of the common method of preparing hydrate of baryta by decomposing sulphid of barium with black oxyd of copper, ALEX. MUELLER proposes to substitute commercial zinc-white for the copper oxyd. The only difference in the conduct of this, and the ordinary process consists in retaining a portion of the solution of sulphid of barium in order to precipitate some zinc oxyd which is dissolved by the baryta water. Or it might be well to decompose the last portions of the sulphid of barium with hydrate of copper (by adding sulphate of copper), since an excess of this could readily be recognized by its blue color. If by oxydation the sulphid of barium had become mixed with any of the thionic acids, the crude hydrate prepared from it should be ignited, after evaporation, with a little nitrate of baryta and the sulphate of baryta thus formed removed by filtration.—*Journal für praktische Chemie*, lxxxii, 52. F. H. S.

II. GEOLOGY.

1. KENTUCKY GEOLOGICAL SURVEY.—1. *Report on the Fossil Flora and of the Stratigraphical Distribution of the Coal in the Kentucky Coal Fields;* by LEO LESQUEREUX, Palæontological Assistant. — 2. *Topographical and Geological Report of the Country along the outline base line following the western margin of the Eastern Coal field of the State of Kentucky;* by JOSEPH LESLEY, Jr., Topographical Assistant. Large

8vo, pp. 333–498.—These two divisions of the Report of the Kentucky Geological Survey, have reached us in advance of publication, thanks to the kind attentions of Dr. Peter, who, since the death of the lamented Owen, is charged with the publication of the work. Mr. Lesquereux here presents the results of his critical studies into the distribution of the fossil plants of the coal strata, in the various coal beds, confirming the palæontological evidence by comparative lithological sections of actual superposition. He is thus enabled to show the true relation of the strata in the different coal fields of Kentucky (as far as his observations have extended) with those of other parts of North America. This has been a great desideratum in American geology, and no observer has enjoyed greater opportunities for exact study in this line of research or has possessed in so eminent a degree the peculiar knowledge which is essential to a correct solution of the problem.

Mr. Lesquereux does not recognize the justness of the division which is often made between the upper and lower Coal measures, i. e., those above from those below the conglomerate, and sees no good reasons for calling the latter 'false coal measures.' 'If,' he says, 'it is based on the fact that the inferior coal beds are not generally found over the whole extent of the coal fields of America, the same can be said of the coal strata between the Mahoning and the Anvil Rock Sandstone, and particularly of the upper coal measures above the Anvil Rock. If this separation is made, from the thickness and extent of the great deposit of sandstone named *conglomerate*, or from its composition of coarser and more pebbly materials, the same reason for a further separation of the coal measures might be found in the thickness, extent, and composition of the Mahoning, and even of the Anvil Rock Sandstones. A separation of the inferior coal beds from the higher measures associated with them, could only be authorized by a difference in the vegetation of which the coal has been formed, and consequently in the species of plants found in the shales. But this difference does not exist, as we shall see presently. It is, therefore, more rational to take the coal measures in their whole vertical extent, as a single and inseparable formation, dividing them, for the sake of a better understanding, in four different parts.

'1. The coal measures below the conglomerate.

'2. The measures between the conglomerate and the Mahoning sandstone.

'3. Those between this last sandstone and the Anvil Rock.

'4. The upper coal measures above it, with their top still undetermined."

We take much pleasure in here reproducing, with some emendations by the author, the "Table of Comparative Sections" from the Kentucky Report, showing the comparative position of the most important coal strata in different parts of the American coal fields. The author adds:

'The number of the sections could have been much increased; but I have deemed it best to record in this table only those which I consider as perfectly reliable, and of which I have been able to compare the palæontological characters, at least at some of their principal horizons.*

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* All the coal beds, the position of which has been ascertained by such palæontological evidence, are marked by a (*).

'The remarkable analogy of distribution of the coal strata, as indicated by these sections, is thus put in evidence, and cannot be ascribed to any ideal system. The order of superposition of the different sections is established on lithological and palæontological characters. But it is only by palæontology that the equivalency of the coal strata has been, and can be, established in distant parts of the same basin, and especially in separate coal fields. Therefore palæontology, as applied to the identification of coal beds, can no longer be considered as a chimerical notion. Its practical advantage is at once evident. And I have no doubt that, as soon as it is generally received as a guide in the examination of our coal fields, the harmony of distribution of the coal strata will become more and more striking.

'The sections made in Kentucky indicate a remarkable increase in the thickness of the measures, especially of the sandstone strata towards the southeast. It would have been interesting to compare sections made in the coal fields of Virginia, south of Charlestown, and to ascertain how far this progression of thickness is continued. It was impossible to get reliable data on the distribution of the coal in that country. From what I have seen myself, many years ago, and especially from sections on Coal river, kindly communicated by Dr. I. H. Salisbury, it appears that coal No. 1, with its members B and C, and perhaps No. 2, subdivides forming as many as eight different strata. A similar development is seen in the north anthracite basin, of which Wilkesbarre is the center. In the same basin we find another analogy in the constant decreasing of the measures from Wilkesbarre to the eastern edges of the basin at Carbondale.

'It was impossible to get any well determined data from the other basins of the Anthracite coal fields. The disturbances of the strata are so marked in innumerable flexures that dynamical geology has failed, thus far, to give any indication touching the general distribution of the veins. From palæontological evidence I am satisfied that the highest coal of the Pottsville and Tamaqua basin is the equivalent of our No. 4, and that the measures do not ascend higher in that part of Pennsylvania. It is certain also that the *big* or *mammoth vein*, so generally worked in the same basin, is the equivalent of No. 1B.

'The 1st section represents the distribution of the coal strata in Union county, Kentucky, and is perfectly correct. It was made by Dr. D. Dale Owen, from borings and repeated measurements.

'Section 2d is the record of the Holloway boring at Henderson, and is also perfectly reliable for the place of the coal strata in this part of Kentucky.

'Section 3d is from Mr. J. P. Lesley's Manual of Coal. It was condensed from such numerous and authentic records as the best geologist can obtain.

'Section 4th is another general section of Pennsylvania copied from the final geological report of the State of Pennsylvania. In comparing these four general sections, it becomes evident that the essential coal strata, viz: coal 1B, C 4th, and C 11th, come under the same horizon. The difference in the intermediate strata is not of material importance. In the section of the Pennsylvania survey there is a distance of 480 feet marked as barren of coal, contradictory to local sections of the same Report, which

TABLE OF COMPARATIVE SECTIONS OF THE COAL MEASURES OF THE UNITED STATES; BY LEO LESQUEREUX.

Scale approximately 1 inch for 200 feet. In the Coal seams each line indicates one foot without reference to scale.

show generally one workable bed, the *Elklick coal*, and sometimes two, above the Mahoning Sandstone. Mr. Lesley's section has marked the place of these strata. In the same section of the report, there is a group of veins, called the Mercer coal, which has no equivalent in the other sections, or which is represented only by a thin coal. Palæontology alone can decide whether this Mercer coal is, or is not, the equivalent of No. 1B and its subdivisions, as it appears to be.

From all the local sections of the Pennsylvania Survey, two ascertained data are especially worth mentioning. 1st. The reliability of our Curlew limestone, which, in Pennsylvania, is called Freeport limestone, and is generally placed 6 to 15 feet above our No. 3 coal: 2d. The consistency of the *ferriferous* limestone between No. 1B and No. 2 in the place occupied by our coal 1C. It lies, as in Kentucky, 10 to 40 feet above No. 1B, and is generally accompanied by calcareous iron ores.

Section 5th, at Yellow creek, is given from measurements of Dr. Newberry, in his railroad survey, and from my own palæontological examination. The distances between the coal strata are said to be too great.

Section 6th was made at Buena Vista and Greenup furnaces from my own measurements, compared with five different sections, kindly furnished by Mr. John Means.

Section 7th, made at Mount Savage, is exact, as far as measurements by pocket level can be relied upon.

Section 8th was taken at West Liberty, first by Dr. D. D. Owen, and afterwards by myself. The upper part of No. 1B crops out in the bed of the river, and it was impossible to ascertain at what distance this member, one foot thick, is placed from the main 1B. The distance, 71 feet, to No. 2, is too short by, probably, 20 to 30 feet.

Section 9th, at Jackson, only shows No. 3d coal as a streak of coal 6 inches thick. The section was followed along a steep ravine, from the bed of the river to the top of the highest hill. Though this coal was covered, and its palæontology was not ascertained, I have no doubt of its identity with the Haddock's coal, our No. 3, which is worked in the vicinity, 275 feet above No. 1A.

Section 10th, at Peach Orchard, was ascertained from measurements and palæontological data. Coal 3d is only marked by a bed of hard fire clay, nearly limestone, or bastard limestone, and a streak of coal, and coal 4th is replaced by fire clay and iron ore, just at the base of the Mahoning Sandstone, which tops the hills, 520 feet above low water of Louisa river, and is conglomeratic at its top.

Section 11th, at Warfield, is made from No. 1A coal, at the top of the boring to the highest hill, 740 feet, where all the coal strata are opened, nearly on the same vertical line. I refer the cannel coal vein, said to be 8 feet thick, to No. 3, and not to No. 4, because it is not placed just below the Mahoning Sandstone, but separated from it by about 90 feet of measures, apparently shales and iron stone. I could find no trace of coal 4th; but the top of the hill is covered, except where the Mahoning Sandstone appears in perpendicular cliffs, with a nearly impenetrable thicket of brambles, which rendered close researches impossible.

Section 12th was taken at Wilkesbarre, in the center of the north anthracite basin. This and the following sections are copied from the report of the Pennsylvania survey, and were especially compiled from borings and observations made by the directors of coal mines. They are entirely reliable. For Wilkesbarre, there is an upper section containing two beds of coal, which would correspond with No. 5th and 6th coals of Kentucky. As this section was not taken from the same place as the lower part, and as I could not see any of the reported coal beds so as to ascertain their palæontology, it is omitted. Some coal beds of unworkable thickness are marked in the section of the Pennsylvania report. But they are not reported by Mr. Lesley, nor were they marked in a section which I obtained of the foreman of the mines.

Section 13th, at Pittston, is remarkable by the separation of coal 2d and C. 3d, each into two beds, separated by ten feet of shales. We have seen the same disjunction of these veins at Chinch creek, and at Whetstone creek, in Greenup county.

Section 14th, at Scranton, is also reported in Mr. Lesley's Manual with some difference.

Section 15th, at Carbondale, was obtained from Mr. Ed. Jones, director of the mines of Archibald. At Carbondale the hills are too low, and contain only the coal 2d. The 3d coal is added from the Archibald's section, which is about the same. In this last place, the distance between C. 1B and 2d is 92, and at Carbondale 95 feet.

The plates illustrating this valuable memoir are not yet published.

The author in his Introductory letter takes occasion to defend himself from the charge of plagiarism made against him by the late Director of the Geological Survey of Pennsylvania in the final Report of that survey. As the alleged offence consisted in quoting the author's own report some years after it was rendered, and with full acknowledgment, it would seem hardly to fall within the requirements of a public apology.

Mr. Lesley, in his 'Topographical Geological Report,' has performed a labor of the greatest practical utility, and one heretofore quite too much neglected in our American geology. It is obvious that the processes of erosion, forming valleys on the courses of all streams, have removed certain portions of the original spread of coal. While therefore the geologist in a general map correctly colors the whole of a given area as coal-bearing, the coal owner finds to his chagrin that much of the coal which ought to be on his lands has 'gone to market.' The only remedy for such disappointments is found in accurate topographical maps in which contour lines at convenient elevations render certain what portions of the several measures remain undisturbed and what are wanting. When to the skill of the topographical engineer is added, as in Mr. Lesley's case, the special knowledge of the geologist, the results are truly valuable.

Kentucky has been fortunate in the character of her scientific corps employed in this survey, and we earnestly hope, in spite of all causes of civil disturbance, this work of peace will receive no obstruction either in its course of completion or in the publication of its results.

On a future occasion we shall return to these reports for the purpose, among other things, of considering Dr. Peter's results on the agricultural side of the survey, both in Kentucky and Arkansas.

2. *Descriptions of new fossils from the Palæozoic rocks of the Western States*, (from the transactions of the Chicago Academy of Sciences,

October 11th, 1859[*]); contributed by J. H. M'Chesney. (2d extract.)—This paper contains descriptions of the following species, which are regarded by the author as new to science, viz., *Orthis Kaskaskensis, O. Kennecotti, Athyris intervarica, A. ultravarica, A. obmaxima, A. obvia, A. perinflata, Terebratula bisacula, T. subretziaforma, T. geniculosa, Rhynchonella Parvini, Spirifer Racinensis, S. clavatula, Rensellaeria Condoni, Pentamerus bisinuatus, P. trisinuatus, P. crassoradius, P. arcuosus, Ambonychia neglecta, Nucula obsoleta, Murchisonia archimedia, Pleurotomaria bicarinata, Platyceras Quinceyensis, Bucania crassolare, B. pervoluta, Orthoceras Laphami, O. Scammoni, O. Hoyi, O. lineolatum, O. cameolare, O. irregulare, O. nodocostatum, O. striœlineatum,* and *Eucalyptocrinus armosus.* The last mentioned species is also illustrated by two wood cuts.

The descriptions of these fossils, although in most cases rather brief, are generally sufficient for the preliminary indication of species. In the selection of names for his species, however, the author has been in some cases rather unfortunate, several of those used having been preoccupied. For instance, Goldfuss published a *Nucula obsolata*—(Petrefact. Ger. II, p. 151). Again Sowerby published a *Pleurotomaria* under the name of *Trochus bicarinatus* in 1818 (Min. Conch., 3d, p. 39), which was referred to the proper genus by Morris in 1843. The same name (*bicarinata*) was also applied to another species of this genus by Koninck in 1843, (Ap. D'Omal. Prec. Elem. Geol. p. 517) and to still another by Munster (Goldf. Petrefact. 3d, p. 72.) The name *Orthoceras lineolatum*, was used by Phillips in 1841, (Pal. Foss., p. 111). Again the specific name *irregulare* was used by Munster for an *Orthoceras* in 1840 (Bietra. zur Petref. 3, p. 100). This criticism applies also to our author's former paper (New Palæozoic fossils,[†] &c.). For instance the name *Leda gibbosa* and *L. polita*, had both been preoccupied by Sowerby for recent species; and *Nucula cylindrica* was used by McCoy for a Carboniferous species in 1844 (Carb. Foss. Ireland, p. 69).

Several of the species described in these papers appear to us *very* closely allied to old and well known forms, though it is but reasonable to infer that the author's means of judging in regard to their difference or identity should be more reliable than an opinion formed from merely reading his descriptions.

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[*] The date here given is not that at which this paper was *published*, but the date at which the paper from which it purports to have been extracted was *read* before the Chicago Academy. The copy before us was received on the 15th of Feb., 1861, which is probably as early as it was received elsewhere. We mention this fact in order to call the attention of authors to the importance of giving the date of *publication*, as well as that at which their papers were presented, where they are distributed in advance of the regular issue of the proceedings or transactions of the body before which they were read; for it must be borne in mind that it is not to the date of *presentation*, but to that of actual *publication*, (i. e., general distribution amongst laborers in the department of science upon which it treats) that we must go in all cases of disputed priority. We must be excused for reiterating so frequently this important point of scientific ethics, since there still appears to be, after all that has been said, an imperfect comprehension of its force and importance.—Eds.

[†] See this Journal, [2], xxix, 285.

III. BOTANY.

1. *Flora Hongkongensis: a Description of the Flowering Plants and Ferns of the Island of Hongkong;* by George Bentham, V.P.L.S. *With a Map of the Island. Published under the Authority of Her Majesty's Secretary of State for the Colonies.* London: Lovell Reeve. 1861. pp. 482. 8vo.—We have already noticed Harvey and Sonder's Cape Flora, and Grisebach's Flora of the British West Indies, each of which have completed a first volume. The present work is the third of this series of British Colonial Floras, upon a new and simple plan, compact in form, written in English throughout, authorized and supported by the British Government. The Colonial department pays a very moderate recompense to the authors, and turns the work over to a publisher upon such terms as to render the volume generally accessible to working botanists and colonists. This is a much wiser as well as vastly more economical plan of government patronage to scientific publication than that adopted in this country, one which secures that the publications are just what is wanted and that they reach the hands which are to use them, and not others—one which, when our present task is done and we again cultivate the arts of peace, we might profitably adopt. The present work is by a master-hand; for Mr. Bentham is one of the most experienced, industrious, and judicious of systematic botanists. The island of Hongkong has an area of scarcely thirty square miles, its general aspect is bleak and barren; yet it has already yielded about a thousand phænogamous species. "At a first glance," as the author observes, "one is struck with the very large total amount of species crowded upon so small an island, which all navigators depict as apparently so bleak and bare;—with the tropical character of the great majority of species, when botanists agree in representing the general aspect (derived from the majority of individuals) to present the features of a much more northern latitude;—with the large proportion of arborescent and shrubby species, on a rocky mass where the woods are limited to a few ravines, or short narrow valleys half monopolized by cultivation;—and with the very great diversity in the species themselves, the proportion of orders and genera to species, and the comparative number of monotypic genera, being far greater in the Hongkong Flora than in any other Flora of similar extent known to me. The very large number of endemic species,—of species known to us only from the island—is probably occasioned by our ignorance already alluded to, of the vegetation of continental South China."

A fitting acknowledgment is given for the important contribution to this Flora furnished by the botanical collection (of above 500 species) made by Charles Wright, as botanist of the U. S. North Pacific Exploring Expedition under Captains Ringgold and Rodgers, duplicates of which were obligingly and most properly furnished by direction of the Commander and the enlightened Secretary of the Smithsonian Institution.

In aid of the colonial botanists or amateurs who may use this Flora, the author has prefixed (with some minor alterations) the admirable brief outlines of Botany and Glossary prepared for his popular British Flora.

In these Outlines the subject is regarded, not from the morphological or the physiological, but from the descriptive point of view. It opens with a statement of the nature and design of a Flora, and of what a botanical description ought to be.

"These descriptions should be clear, concise, accurate and characteristic, so as that each one should be readily adapted to the plant it relates to, and to no other; they should be as nearly as possible arranged under natural divisions, so as to facilitate the comparison of each plant with those nearest allied to it; and they should be accompanied by an artificial key or index, by means of which the student may be guided step by step in the observation of such peculiarities, or characters, in his plant as may lead him, with the least delay, to the individual description belonging to it.

"For descriptions to be clear and readily intelligible, they should be expressed as much as possible in ordinary well-established language. But, for the purpose of accuracy, it is necessary not only to give a more precise technical meaning to many terms used more or less vaguely in common conversation, but also to introduce purely technical names for such parts of plants or forms as are of little importance except to the botanist. In the present chapter it is proposed to define such technical or technically limited terms as are made use of in these Floras.

"At the same time mathematical accuracy must not be expected. The forms and appearances assumed by plants and their parts are infinite. Names cannot be invented for all; those even that have been proposed are too numerous for ordinary memories. Many are derived from supposed resemblances to well-known forms and objects. These resemblances are differently appreciated by different persons; and the same term is not only differently applied by two different botanists, but it frequently happens that the same writer is led on different occasions to give somewhat different meanings to the same word. The botanist's endeavors should always be, on the one hand to make as near an approach to precision as circumstances will allow, and on the other hand to avoid that prolixity of detail and overloading with technical terms which tends rather to confusion than to clearness. In this he will be more or less successful. The aptness of a botanical description, like the beauty of a work of imagination, will always vary with the style and genius of the author."

These Outlines are throughout so well sketched, and so worthy to be regarded as of standard authority, that we must still venture a criticism or two, looking to their possible improvement.

In the first place, referring to paragraphs 8 and 88, we must dissent from the proposition that the subject of *homology* does not belong to "*morphology* in the proper sense of the term;"—unless, indeed, morphology relates simply to form in the lowest sense, to mere shape, arbitrarily viewed,—which would belittle the subject down to mere terminology, and empty that of all scientific interest. If the comparison even of a perfoliate or clasping with a cordate leaf, or of membranaceous or coriaceous with thickened leaves, such as those of a Houseleek, a Mesembryanthemum and an Aloë, falls within the province of morphology, surely so also must the comparison of an ordinary leaf with a cotyledon, with a bulb-scale, a bud-scale, and no less with a sepal, a petal, a carpel, &c. In the latter we merely trace morphological relations of the very same kind somewhat further and higher. The relation of a leaf as foliage to the scale of a bud, or to the thorn of a Barberry is clearly of the same category as its relation to a sepal or a petal,—the latter, as we regard it, bringing in no new idea, and requiring no new point of view.

Next, *Quincuncial* imbrication is defined by Mr. Bentham to be that arrangement in which "one petal is outside, an adjoining one wholly inside, the three others intermediate and overlapping on one side." But why give this name to a mixed form, to that which is merely convolute æstivation deranged by one of the five petals getting both edges under? And why change the uniform usage from De Candolle's *Théorie Élémentaire*, if not earlier, down to the present time, which defines the quincuncial mode as having two members exterior, two interior, and one with one edge overlapping its neighbor and the other overlapped;—an arrangement which especially merits a distinguishing name, since it is the normal imbrication in a pentamerous perianth, answering as it does to $\frac{2}{5}$ phyllotaxis. So that current usage and reason tell against the innovation.

In the third place, we are equally inclined to demur to the proposed modifications of the sense of the terms *perigynous* and *epigynous* (paragr. 140), Mr. Bentham restricting the former to those cases in which the petals, &c., are adnate to a perfectly free calyx, as in the Cherry, and applying the latter in cases where the calyx, equally bearing the petals, &c., is adnate even merely to the base of the ovary, if only the adhesion reaches above the level of the insertion of the lowest ovule;—which would make most Saxifrages epigynous. Besides the etymological objections, and the inconvenience of a change, the new definitions seem to us to be at least as ambiguous as the old in practice; and it is not surprising that they are not uniformly adopted in the Hongkong Flora itself.

Finally, as to paragr. 166, we are not much better satisfied with the definition that the radicle is the "base of the future root," than with the original statement that it *is* "the future root." To us nothing in botany is clearer, or more patent to observation during germination, than that while the radicle is, if you please, "the base of the future root" inasmuch as it is that from which the root proceeds, it is itself the first internode of stem. This view, to which morphological considerations and observation of the development long since brought us, appears to be generally adopted by the French and German botanists, but not by the English. If the radicle universally failed to elongate, as in Monocotyledons, and in the Pea, Oak and others with hypogæous germination, this organ might be deemed to be merely the base of the future root; but its more usual elongation, in the manner of any other internode, plainly reveals the cauline nature which analogy would also assign to it.

The chapter on Vegetable Anatomy and Physiology is new, is very condensed, and considering that it deals with matters to which Mr. Bentham has never specially attended, is remarkably good and accurate. We merely observe in passing, of paragr. 195, 197, that the distinction between exogenous and endogenous stems, is as obvious *during* the first season, and even at its beginning, as ever afterward, and it is then that the purely systematic botanist will more commonly have occasion to examine the structure in this regard; of §198^5, that "the liber or inner bark" is by no means always "formed of bast cells;" of §200, that we cannot accept the statement that "in the leaf the structure of the petioles and principal ribs or veins is the same as that of the young branches of which they are ramifications," at least in any sense in which the sentence would be understood by the learner. Paragr. 207, that roots grow in length at the extremities, "in proportion as they find the requisite nutriment," might imply the popular fallacy that they grow directly by means of what they take in from the soil, which surely they do not, unless they live in the manner of *Fungi*. To say that the starch, &c., in a tuber or in a seed "*appears* to be a store of nourishment" for the early growth of the buds or the embryo, is a remarkably over-cautious statement (how could these grow without some store of elaborated matter to feed upon?): nor does the consideration that similar accumulations in the pericarps of many fruits "perish long before germination," and so do not nourish the embryo, afford to us any presumption to the contrary, even if we could not conceive—as we readily can—of other final causes, some of them important to the continuance of the species, thereby subserved.

The fourth chapter, on the Collection, Preservation, and Determination of Plants, and upon Aberrations from the ordinary type or appearance, is most excellent. A. G.

2. *Annals of the Botanical Society of Canada*, Vol. I, part I, (Dec., 1860–March, 1861), 4to, pp. 60.—This Society enters upon its career with zeal and spirit. These Annals open with the history of the Origin of the Society, and of the proceedings attending its formation, the Laws, and the Proceedings of the earliest meeting. Professor Blackie of Nashville, Tennessee contributes a medico-botanical paper upon *Cornus florida;* Mr. Schultz one upon the Botany of the Red River Settlement and the old Red River Trail; Mr. Drummond, Contributions to the local flora of Kingston, the head-quarters of the society; Mrs. Dr. Lawson, on the silkworm and other fibre-yielding insects, and the growth of their food-plants in Canada; a note on the Hubbard Squash, by Mrs. Thomas Briggs, Jr.; what to observe in Canadian Lichens, by Dr. Lindsay; on the genus *Graphephorum* and its synonymy, by Prof. Gray of Cambridge; List of Plants collected on the Island of Anticosti and the coast of Labrador, by Mr. J. Richardson; and some other short communications. A. G.

3. *Journal of the Proceedings of the Linnæan Society*, No. 19, (1861) gives the remainder of Mitten's *Enumeration of the Hepaticæ of the East Indies*, and a good part of another of the *Præcursores ad Floram Indicam*, by Drs. HOOKER and THOMPSON—the *Cruciferæ*. Here Dr. Hooker gives,—what was much needed,—a new distribution of the genera of *Cruciferæ*, so far as represented in India. The primary divisions, four in number, rest upon the pod, whether jointed or jointless, indehiscent or dehiscent, and in the latter, whether compressed parallel or contrary to the partition. The tribes, eleven in number, rest upon the shape of the pod, the arrangement of the seeds, and, rather subordinately, upon the cotyledons. The whole makes a much improved natural arrangement. Under *Barbarea* we are not surprised to see *B. arcuata* and *B. præcox* reduced to *B. vulgaris*, nor to see *Turritis* reduced to *Arabis*. A. G.

4. *Class-Book of Botany; being Outlines of the Structure, Physiology, and Classification of Plants;* with *a Flora of the United States*

and Canada. By Alphonso Wood, A.M., Principal of Female Academy, Brooklyn. New York, Barnes and Burr, &c., 1861, pp. 832, large 12mo. —This is a second and enlarged edition of the Class-Book published in the year 1845, a book which has been widely used in Botanical instruction, and which, so far as the merits of a work may be tested by the sales, must be allowed to have stood the criterion. It was natural that the author should wish to extend and improve it, and the publishers had every inducement to incite and second his endeavors. The reproduction, although designed to cover the same ground in education, is of greater pretension than the original issue. It claims a higher character, even the authority due to original investigation and competent, independent judgment. Wherefore the work invites an examination such as it would be hardly fair to apply to the former production, although in that, also, the author "distrusted every source of information except our own personal inspection," and claimed more complete, scientific, and particular acquaintance with our Flora than would have been expected. Sixteen years more of botanical study, however, should largely substantiate all similar claims. The arrangement of the page is good, and so also the choice of letter for the names which should conspicuously strike the eye, taking as it does after Dr. Chapman's book and other models. The whole appearance of the volume is commendable, or would be so if it were not seriously disfigured by clerical and typographical errors. These are annoying enough everywhere, but are especially injurous in a school-book. We know too well how misprints will lurk in a page, escaping the eye of the most attentive authors and proof-readers; but those of the present volume are beyond excuse, both for the number and the character of such blemishes. Justice to the printer constrains us to add, that for very many of them he can hardly be the responsible party. Especially must this be the case with a crowd of misspelled botanical names,—although the eyes of a competent proof-reader must have dilated with wonder that an author should uniformly write *dilitata* for *dilatata*, *pigmæa* for *pygmæa*, designate the genus *Salisburia* by the adjective name of *Salisburiana*, and the like. So also of *Simplocarpus*, *Arctostaphilos*, *thetipteroides*, which again are matched by counterpart errors in *Psylocarya* (with a species *rhyncosporoides*), *psylostachya*, *Bryzopyrum*, *rhyzophilla*, *Peplys*, *distychum*, *distychophylla*, and so on; to say nothing of *pedicillaris*, *psiticina*, *Chamæchrista*, *Crotallaria*, *precox*, *speciocissima*, *Eriophila*,—the latter transforming a lover of spring into a lover of wool! But the pupils of the Brooklyn Female Academy who can read the Greek letters in which the derivation of some of the above words is given, or who have a Latin dictionary at hand, for some of the rest, can readily be taught to make the requisite corrections.

So also we have *Limonum* for *Limonium*, *Nyctelæa* for *Nyctelea*, *Lipachys* for *Lepachys*, *Nasæa* for *Nesæa*, *Achmella* for *Acmella*, *Naumbergia* for *Naumburgia*, *Phytolacca decandria* and *Stuartia pentagynia*, and *Andromeda polyfolia* for *polifolia*, as if this specific name meant many-leaved and was a hybrid of Greek and Latin (like *leptoculmis*, p. 146). It is not so surprising that he should fall into the mistake of writing the genus *Polianthes* of Linnæus, *Polyanthes*, and so give a derivation to match, although our author might have placed some confidence in Linnæus, Endlicher, &c.; and even the common books, such as Loudon, give the correct orthography and derivation.

To copy names correctly from other books, or to see that they are correctly printed, requires no high order of talent. Wherever the fault lies, there was no need to transform the connective of the anther (Latin *connectivum*) into connect*ile*, nor the name of Payer into Peyer, nor that of Duhamel into Dunham, nor that of the prince of agronomical chemists, Boussingault into B*r*oussingault, and the familiarly cultivated ornamental plant which commemorates him into *Broussingaultia.* To justify our opinion that these are not simply errors of the press, and to give a crowning example of what bad spelling may lead one to in the end, we turn to page 292, and read:

"SEGREGATIA, Brongn. (Name referring to the *segregated* clusters of the interrupted spikes)."

Now the name in question is *Sageretia*, given in honor of M. Sageret, a distinguished French horticulturist and vegetable physiologist, as very common botanical works would have informed Prof. Wood. But having cautiously "distrusted every source of information except that of our own personal inspection," and a careless inspection of the name having led to a very queer orthographical result, and not having the original memoir of Brongniart at hand to explain the derivation, our author, it seems, could do no less than to invent one, to suit his misreading.

From the spelling (which even as to English words has some peculiarities, e. g., *Purselane* for *Purslane*), we might pass to the pronunciation, and we had marked a goodly number of names wrongly accentuated; also misapprehended etymologies. But it is not worth while to particularize them. Nor have we room now to discuss botanical points which numerously present themselves to our notice, as they would open too wide a field for remark. We freely allow that the "principle" which "has compelled" the author "to disallow the claims of many reputed species of the best authors" is applied with more probability in other cases than in the first instance the book affords, where *Sarracenia flava*, *rubra* and *Drummondii* are reduced to varieties of one species! while a leaf of one of them with a conspicuous wing is raised to equal rank, as if all three species did not produce some leaves, especially the earlier ones of the season, with a broad wing.

It is not likely that the various *new species* of the volume will bear scrutinizing. (As to a new *Senecio*, where did Prof. Wood find the specific name *anonymus?*) Some of them owe their position to misconceptions or to want of proper and needful books; and others to what looks like willful ignoring of a well-known American work, viz., Dr. Chapman's Flora of the Southern United States, published in New York in the spring of 1860, while Prof. Wood's preface bears the date of December of that year, and the edition was printed, from electrotyped plates, in the spring of the present year. Of some of these new species specimens were presented to our author by Dr. Chapman himself; as, for instance, Dr. Chapman's interesting *Goodyera quercicola.* This,—showing thereby a wonderful knowledge of the order—Prof. Wood publishes as a new *Platanthera! P.? quercicola.* As it happens, it is not a *Goodyera*, though so nearly related as to excuse Dr. Chapman in appending it to that genus;

and Lindley published it long ago under the name of *Physurus querceticola.*

We are curious to learn upon whose authority the leaves of *Victoria regia* are said to attain the diameter of 12 feet! We should particularly like to see *Schizæa pusilla* growing in Western New York, and trust that Mr. Timothy Wetmore will have the kindness to indicate the locality.

What can the statement mean that *Trisetum molle* (really only a luxuriant form of *T. subspicatum*) "scarcely differs" from *T. purpurascens*, Torr., which is a good *Avena*. To common eyes, no two grasses of the same tribe could be much more different. Why did not our author's "personal observation" teach him that his "*Fimbristylis distachya*, Chapman," is the same as the *Hemicarpha subsquarrosa*, on the opposite page, as Dr. Chapman was sharp enough to see before he printed his book.

We might go on a long while with such criticisms; and the introductory part would supply text for a long series of animadversions. Still we find throughout the volume evidences of much painstaking, of a genuine love for the science, and of commendable skill in smoothing the student's way to a moderate acquaintance with it, especially by means of analytical tables and keys. Well-meant endeavors should not be judged too severely; and we trust that our remarks are not over-critical or captious.

A. G.

5. *The Natural History Review, a Quarterly Journal of Biological Science*, No. I, January; II, April, 1861.—We would call attention to this new English Scientific Journal, which is published by Messrs. Williams & Norgate, London, (B. Westermann & Co., New York, being the American booksellers on the title-page*), at four shillings per number, the annual subscription twelve shillings sterling. The editors are, for Physiology and Histology, Messrs. Carpenter, McDonnell and Wright; for Systematic Zoology and Distribution, Messrs. J. Reay Greene and P. L. Sclater; for Anatomy and Embryology, Messrs. Busk, Huxley and Lubbock; for Botany, Messrs. Oliver (the promising new Professor in the London University), and Currey; for Palæontology, Messrs. Huxley and Wyville Thomson.

"In undertaking the conduct of the Natural History Review the Editors" state that they "propose to establish a Quarterly Critical Journal of Biological Science, which, without interfering with existing Scientific Periodicals, shall stand in the same relation to Naturalists and other persons interested in biological inquiries, as that which is occupied by the ordinary quarterly Reviews in respect of Men of Letters and the General Public.

"They desire, in addition, to offer to all whom it may concern, a means of discussing the general problems suggested by the progress of biological investigation in a philosophical spirit, and solely with reference to scientific considerations. The Editors will not refuse an original communication on the ground of any scientific opinion expressed in it.

"The contents of the Natural History Review will be divided into the following sections: I. Reviews; II. Original Articles and Reports; and III. Bibliographical notices and Miscellanea.

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* Price in New York, $1.20 per number. $3.60 per annum.

"The first section will embrace criticisms of all important new biological works published either in this country or abroad. The second will comprise such original papers, as may be of sufficient importance to deserve publication, though they may not be of a nature to demand a place in the Transactions of a Scientific Society. Careful Reports upon the present conditions of particular branches of Natural History will form an additional important constituent of this section.

"In the third section will be comprised notices of all the Papers on biological subjects read before Scientific Societies; a bibliographical record of the various serial publications and works on Natural History that have appeared during the quarter; and, finally, miscellaneous notes.

"While the last section will form a record of the daily progress of Science, of the greatest importance to the working Naturalist, the Editors trust that the two former will contain many communications of value, not only to the man of Science, but to that large and increasing number of persons who take an interest in the results of the investigations of the professed Naturalist.

"As the wide extent of Biological Science renders it impossible for any man to be largely acquainted with more than two or three of its branches, the Editors have divided the labor of collecting and supervising the requisite materials according to the scheme which heads this prospectus; but they do not hold themselves responsible for the opinions expressed in articles to which their names are not attached, and they particularly request that all communications intended for the Journal may be addressed to the Publishers, the words 'Natural History Review' being written upon the outside cover."

The *Reviews* in the first number are, 1. Of the *Flora Brasiliensis* of Von Martius,—in which we discern the hand of one of the best botanists of our time;—2. Berkeley's *Outlines of British Fungology*;—3. *The Mammals of Amoorland;* Leydig's *Natural History of the Daphnidæ*, reviewed by Mr. Lubbock; *On the Natural* Position of the Group *Protozoa*, a review of several recent works relating to or touching upon these simple organisms. The *Original Articles* are one by Mr. Lubbock *On Spærularia Bombi;* one by Dr. McDonnell *On an Organ which in the Skate appears to be the Homologue of the Electrical Organ of the Torpedo;* by Mr. Wright, *Notes the Anatomy of the Alimentary System of the Axolotl* (*Siredon Mexicanum*); and finally, one which has naturally attracted unusual attention, by Prof. Huxley, "*On the Zoological Relations of Man with the Lower Animals.*"

Of No. II, the *Reviews* are on Prichard's *History of Infusoria*, fourth edition; and on the *Primitiæ Flora Amurensis.* The *Original Articles* are one *On the Species and Genera of Plants, considered with reference to their practical application to Systematic Botany*, by George Bentham,—an article full of wisdom. *On the Serial Homologies of the Articular Surfaces of the Mammalian Axis, Atlas, and Occipital Bone*, by Dr. John Cleland. *On the Crania of the most Ancient Races of Men*, by Prof. Schaafhausen of Bonn (from Müller's Archiv, 1858), *with Remarks, and Original Figures taken from a Cast of the Kanderthal Cranium*, by George Busk;—an extremely interesting article. *The Sensory and Motor Functions of Nerves*, by G. H. Lewes. *General Results of the Study of the Typical Forms of Foraminifera*, &c., by Dr. Wm. B. Carpenter—particularly interesting, as are other articles in this and the former number, on account of the manner in which the subject is treated in relation to the Darwinian hypothesis. *On the Affinities of the Brain of the Orang Utang*, by Dr. Rolleston, the new Linacre Professor of Anatomy at Oxford; a well-considered and every way most admirable communication. Analysis of or comments on these articles would be here out of place.

The *Bibliographical Record* occupies about 36 pages in each number. It will be very useful. This periodical was much wanted. It challenges attention; and deserves, and we hope will receive, a larger support than has usually been given to British Scientific Journals of a high order.

IV. ASTRONOMY AND METEOROLOGY.

1. *The recently discovered Asteroids.*—For convenience of reference, we place on record a summary of the asteroids discovered since 1858.

(57) Mnemosyne, discovered on the evening of Sept. 22, 1859, by Dr. R. Luther of Bilk. Its brightness is that of a star of the 10th magnitude.

(58) Concordia, discovered March 24, 1860, by Dr. Luther of Bilk. Its brightness is that of a star of the eleventh magnitude.

(59) ———, discovered Sept. 12, 1860, by M. Chacornac at Paris. Its brightness is that of a star of 9th to 10th magnitude, and this is the sixth planet discovered by M. Chacornac.

(60) Danaë, first seen Sept. 9th, 1860, by M. Goldschmidt of Paris, and recognized as a planet Sept. 19th. Its brightness is equal to a star of the 11th magnitude.

(61) Titania, discovered by Mr. Ferguson, at Washington, on the night of Sept. 15th, 1860. It appears as a star of the 11th magnitude.

(62) Erato, discovered at Berlin by Dr. Förster and M. Lesser. First observed Sept. 14th, 1860, but supposed to be Chacornac's planet (59). The continuation of the observations up to Oct. 10th, showed that this was a distinct planet. Its brightness is that of a star of the 11th or 12th magnitude.

(63) Ausonia, discovered at Naples, by M. De Gasparis, Feb. 10th, 1861. It appears as a star of the 10th magnitude, and is the eighth planet discovered by M. De Gasparis.

(64) Angelina, discovered at Marseilles, by M. Tempel, March 2, 1861.

(65) Maximiliana, discovered at Marseilles, by M. Tempel, March 4th, 1861.

(66) Maia, discovered at Cambridge, Mass., by Mr. H. P. Tuttle, April 9th, 1861. It appears as a star of the 13th magnitude.

(67) Asia, discovered at Madras, India, by Mr. N. R. Pogson, April 17, 1861. It appeared as a star between the 11th and 12th magnitudes, and this is the fourth planet discovered by Mr. Pogson.

(68) Leto, discovered at Bilk, Germany, by Dr. Luther, April 29th, 1861. It appeared as a star of the 11th magnitude. This is the tenth planet first discovered by Dr. Luther.

(69) Hesperia, discovered at Milan, by Sr. Schiaparelli, April 29th, 1861. It appeared as a star of the 11th magnitude.

(70) Panopea, discovered at Paris by M. Goldschmidt, May 5, 1861. It was between the 10th and 11th magnitudes. This is the fourteenth planet discovered by M. Goldschmidt.

We append the elements of several of these planets, according to the most recent computations:

Berlin mean time.	(57) Mnemosyne 1860, Jan. 1·0.	(58) Concordia, 1860. Jan. 0·0.	(59) ——— 1860, Oct. 0·0.	(60) Danae, 1860, Sept. 29·0
L	28° 35′ 25″·6	162° 16′ 26″·9	9° 19′ 40″·1	345° 42′ 33″ 8
π	52 53 13 ·0	177 55 57 ·8	18 56 24 ·4	340 8 37 ·7
☊	200 5 25 ·1	161 21 35 ·7	170 18 54 ·6	334 19 6 ·5
i	15 8 1 ·6	5 2 57 ·8	8 36 50 ·1	18 17 1 ·0
e	0·104116	0·040091	0·118840	0·163084
μ	632″·4633	802″·9694	793″·561	691″·58794
a	3·157288	2·692806	2·714047	2·974675

Berlin mean time.	(61) Titania, 1860, Oct. 1·0.	(62) Erato, 1860, Sept. 24·0	(63) Ausonia, 1861, March 9·0.	(64) Angelina, 1861, April 0·0.
L	355° 30′ 17″·1	5° 1′ 31″·7	178° 35′ 2″·9	170° 42′ 16″·7
π	158 5 38 ·6	40 11 41 ·3	272 54 41 ·9	126 28 10 ·5
☊	187 12 10 ·3	126 57 24 ·5	338 9 23 ·8	311 2 28 ·4
i	4 41 4 ·4	2 14 55 ·3	5 56 23 ·7	1 19 40 ·0
e	0·198641	0·165012	0·131550	0·124830
μ	1024″·1465	636″·320	953″·156	809″·508
a	2·28962	3·14452	2·40194	2·67828

Berlin mean time.	(65) Maximiliana, 1861, March 18·55.	(66) Maia, 1861, May 16·6.
L	194° 15′ 24″·1	183° 21′ 17″·6
π	254 37 32 ·0	43 54 5 ·7
☊	159 9 11 ·9	8 11 41 ·7
i	3 29 14 ·1	3 4 8 ·8
e	0·140705	0·154229
μ	553″·149	820″71
a	3·45231	2 65387

In the above tables,
L represents the mean longitude of the planet at Epoch.
π " the longitude of the perihelion.
☊ " the longitude of the ascending node.
i " the inclination of the orbit to the ecliptic.
e " the eccentricity of the orbit.
μ " the mean daily motion.
a " the semi-major axis of the orbit.

Comparing these elements with those of the asteroids previously discovered, we find a large range in the dimensions and positions of the orbits. The asteroid which is nearest the sun is Flora, (mean distance 2·20) with a period of 1193 days. The asteroid most remote from the sun is Maximiliana (mean distance 3·45) with a period of 2343 days; so that the extreme asteroids differ more between themselves, than the orbit of the earth does from that of Venus or Mars.

The asteroid whose orbit has the least eccentricity is Concordia (eccentricity 0·04); that which has the greatest eccentricity is Polyhymnia (eccentricity 0·337). The orbit of Faye's comet has an eccentricity of only 0·556; so that in respect of eccentricity, the asteroids differ more among themselves than they do from the comets.

The asteroid whose orbit is least inclined to the ecliptic is Massillia (inclination 0° 41′); that whose orbit is most inclined to the ecliptic is Pallas (inclination 34° 42′).

Of the 66 asteroids whose orbits have been computed, 16 have their ascending node in the first quadrant; 24 in the second quadrant; 14 in the third quadrant; and 12 in the fourth quadrant; showing but a slight tendency towards that position of the orbits which the theory of Olbers requires.

2. *The Comet discovered by Mr. Thatcher.*—On the evening of April 4th, 1861, a comet was discovered by Mr. Albert E. Thatcher, an amateur observer in the city of New York. The discovery was made with a telescope of 6 feet focus, and 4½ inches aperture, made by Mr. Fitz. The comet was found in the head of Draco, in R. A. about $17^h\ 33^m$ and N. Dec. 56°. On the morning of April 6th, Mr. Thatcher reported the discovery to Mr. Fitz, and on the evening of the same day Mr. Fitz found it with a comet seeker of 7½ inches aperture; after which he went to Mr. Rutherford's observatory, and found it with Mr. Rutherford's telescope. The comet was nearly circular. Its diameter was about 2′, and it was somewhat condensed at the centre. Its place at 13^h sidereal time, April 6th, as determined by Mr. Rutherford, was A. R. $17^h\ 24^m\ 59^s{\cdot}3$; Dec. 56° 42′ 57″ N. The place of the comet was determined again at the same observatory, April 7th, 9th, 10th, 20th, 26th, etc. On the 10th of April, the brightness of the comet had increased, the nucleus was ill defined, and its diameter about 5′. April 26th the comet was visible to the naked eye, was nearly circular in form, and about 6′ in diameter. Throughout most of the month of May, the comet was visible to the naked eye as a star of the third magnitude.

At the Washington observatory, the comet was observed April 10th, at $10^h\ 10^m\ 20^s{\cdot}6$ mean time, in R. A. $17^h\ 7^m\ 56^s{\cdot}71$, Dec. $+59°\ 26'\ 13''{\cdot}53$. At the Cambridge Observatory the comet was also observed April 10th, 11th, 14th, 18th and onward.

The comet was seen in London by Mr. Parkin, April 29th, and on the night of May 1st with the naked eye.

The monthly notices of the Royal Astronomical Society, published May 13th, contain the announcement of the discovery of this comet by Mr. Thatcher, but without any intimation of its having been seen in any part of Europe before the 29th of April. It is presumed therefore that Mr. Thatcher was the first discoverer of this body.

The elements of this comet have been computed from the Cambridge observations of April 10th, 18th and 29th by Mr. T. H. Safford, assistant at the Cambridge Observatory, as follows:

T.	1861. June 2·8672 m. t. Washington.		
log. q.	9·96488		
π−☊	213°	2′·70	Mean eq. 1861·0.
☊	29	47·45	
i	80	2·62	

Motion direct.

The preceding elements do not bear any very close resemblance to those of any comet hitherto observed. According to these elements, the comet on the 11th of May passed so near the earth's orbit as to include it in its atmosphere; the comet's distance from the earth being at that time about thirty-six millions of miles.

3. *On the Algebraical and numerical theory of errors of observations and the combination of observations.* By George Biddell Airy, M.A., Astronomer Royal of Great Britain. London, 1861. 12mo, 120 pages.—The object of this short treatise is to determine the probable error of the mean of any number of measures of a physical element, together with the most advantageous mode of combining such observations. The conclusions of Mr. Airy differ somewhat from those which for some years past have been extensively adopted by men of science. Legendre demonstrated that when it is required to determine the values of several quantities from a large number of simple equations, the most probable values of the unknown quantities are those which render the sum of the squares of all the errors the least possible. This method, which is called *the method of least squares* has been very generally adopted by men of science throughout the continent of Europe, and also in this country. The result of Mr. Airy's investigation is that the observations should be combined by multiplying each measure by a number which is called the "combination weight;" that we should add together these products of measures by combination weights; and divide the sum by the sum of combination weights. With respect to that combination-weight which is most advantageous, Mr. Airy concludes that that combination is best which gives a result whose probable error is the smallest possible. It is then necessary to determine the value of the probable error in each of the observations. Frequently this cannot be done by any simple rule, because the observations are not all alike. The determination of the value of this probable error must rest very much with the judgment of the observer. Mr. Airy adds "the reader must not be startled at our referring these decisions to his judgment, without material assistance from the calculus. The calculus is, after all, a mere tool by which the decisions of the mind are worked out with accuracy, but which must be directed by the mind."

METEOROLOGY.—

[We present herewith abstracts of several important papers by Prof. Haidinger of Vienna on the subject of meteoric falls in which will be found many new and important views both physical and mineralogical lately announced by this distinguished author:]

1. *Hraschina near Agram.**—One of the most interesting falls of meteorites and for a long time the only one of metallic iron, which had been *witnessed* is that of Hraschina near Agram, May 26th, 1751. It is noticed in Prof. W. S. Clark's paper on metallic meteorites, (Am. Journ. of Sci., 2d Ser., vol. xv, page 15). W. Haidinger at the meeting of the Imp. Acad. of Vienna of April 14th, 1859, produced the Latin document referring to it (which never had been published) and the original German translation; also a *second* document accompanied by two pictured plates, representing the phenomenon as observed at Szigetvár (or Gross. Sziget), 75 miles E. of Hraschina, a paper lately discovered in the Imperial Cabinet of minerals at Vienna. At the meeting of Febr. 3d, 1860, he presented a *third* document, which had been discovered in the archepiscopal library at Agram, describing the phenomenon as seen at Biscupecz near Warasdin, 17½ miles north, a little towards East of Hraschina.

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* *Der Meteoreisenfall von Hraschina bei Agram am 26 Mai, 1751, von* W Haidinger, *Wien,* 1859, *mit* 1 *chromolithogr. Tafl.*

These various documents give some data and explain some relations, which heretofore were doubtful and contradictory.

The meteor, which fell on May 26, 1751, between 6 and 7 P. M., west of Szigetvár (or Gross. Sziget) was at that place first observed as a flash of light *without* any noise, immediately after, it appeared like a flame resembling a tortuous chain, extending directly toward the west and terminating, in the middle height of the air, in a "fireball," which left behind a long tail. On its arrival in the lower strata of the air it appeared like a sparkling fireball of enormous size with a tortuous chainlike tail in the higher regions, the last traces of which faded away at about 10 o'clock P. M. At Biskupecz it was observed as a small cloud, from which some noise was emanating, and which afterwards disappeared.

Two pieces of iron fell a little east of Hraschina, one of 71 pounds, penetrating the ground to a depth of four feet and six inches (not 18 feet as generally stated, owing to an erroneous translation of "ad tres cubitos") and at present preserved in the Imp. cabinet of Vienna, the other of 16 pounds, which had been distributed partly at the place of its fall and afterwards at Pressburgh, and of which every vestige is lost. From the computations of various observations of the meteor it appears that it passed from Neustadt to Hraschina or from north to south from 48° 35′ to 40° 6′ 2″, and from west to east from 28° 18′ to 34° east of Ferro, which requires a motion of the earth 22·8 minutes, until the western meridian occupies the place of the eastern. We have no observation with regard to the velocity of this fireball, but its height just before its fall at Hraschina, as viewed from Szigetvár was between 30° and 35°, equal to an altitude of 43 to 52·5 miles.

Another consideration is very important. The planetary velocity, with which the meteorite entered our atmosphere and remained at about the same height between Neustadt and Hraschina, was checked a little east of the latter place, where the fall suddenly begun. The resistance of the air alone could have produced this result, and a gradual preponderance of the attraction of the earth would have produced in the path of the meteor a far less sharp angle, than that at Hraschina.

Prof. H. calls attention to another matter of great interest, the vast difference between the apparent size of the meteor and its solid contents. A body of 15 inches in diameter at a distance of 75 miles is invisible, yet, the meteor is pictured as if of the size of the sun. The appearance of the chain indicates the time when the solid portions became visible, they are undoubtedly however only the paths of the luminous bodies, and, that they do not form straight lines, is very natural, if we take into consideration, the flat shape of the meteorite, which, by its rapid passage, must have been tossed from one side to the other by the resistance of the air. If the rapid compression of the air is sufficient to annul the cosmical velocity, it certainly can produce the elimination of light, the fiery phenomena. These two points established, as a natural consequence of the same cause, two other phenomena result, which belong to the character of fiery meteors. The solid nucleus of a meteor is not a globe, it passes undoubtedly through the resisting medium, with its centre of gravity foremost, producing at the same time on account of the unequal distribution, a rotation of its mass, which increases in rapidity, whilst the velocity of the motion diminishes in a direct ratio. We can readily conceive therefore, that this rotation can easily produce the bursting of an unequally heated stony mass, coming from a space whose temperature is $-50°$ to $-90°$ C., when heated in a few seconds from its outside red-hot by the compressed air. The rotation would be sufficient also to put the surrounding strata of the air in a whirling motion, the air compressed to such an extent, that it becomes luminous, would by the rotation of the nucleus be driven out tangentially along its globular surface, re-uniting again behind it, where a vacuum had been formed. The fireballs are only visible at a distance, close by only the black stones. In accordance with the above suggested powerful rotation is the breaking asunder of the flat mass in two pieces of 71 and 16 pounds respectively, by the centrifugal power.

The report of the explosion of the Hraschina meteor was heard as far as Warasdin, which would give, taking Hraschina as the centre, an area of nearly 1000 square miles, over which the sound was audible.

In opposition to Chladni's theory, that the explosions of meteors are produced from the centre towards the outside by the bursting of a tough envelope filled with gas, Haidinger suggests a far more simple and natural explanation. Taking into consideration that the meteorites arrive as frequently highly crystalline solids and that they produce their fiery envelope (a real photosphere) during their passage with planetary velocity through our own atmosphere by the condensation of the air before them and the pushing away of the same in all directions perpendicular to their orbit, thus obtaining a rotary motion, by which in the interior of the fireballs a vacuum is produced, there is a possibility that, when the meteor enters the denser strata of our atmosphere, where its velocity is checked or its motion completely stopped, the empty space is suddenly filled by the surrounding air and *thus* the sound produced.

The Hraschina iron was the first, in which the highly crystalline structure of meteoric irons was observed, and Haidinger gives an interesting account of the circumstances, under which this discovery was made. Alois von Widmannstätten, a highly educated and thorough iron master, had a plate of the mass cut off $1\frac{3}{4}$ by 1 inches in size and $\frac{3}{16}$ of an ounce in weight, which he had carefully polished for the purpose of examining its behavior, when exposed to heating. But what a surprise! After the color of the principal mass had passed through the various shades of straw-yellow, brownish yellow and violet into blue, there remained groups of regular triangles of straw-color parallel lines, the blue and violet intervals $\frac{1}{4}$ to $\frac{1}{2}$ line wide, the straw-yellow lines $\frac{1}{6}$ to $\frac{1}{4}$ of this width, in reality, a splendid phenomenon. This was the first observation, and the figures were called "Widmannstätten's figures" in honor of their discoverer. The method, generally in use at present, the etching by acids, was introduced immediately after this discovery.

2. *Leitform*—Or the typical form of Meteorites.*—In a paper on a typical form of meteorites presented at the meeting of the Imp. Acad. of Vienna on the 19th of April, 1860, Director Haidinger suggests some new and very interesting ideas. The paper is accompanied by two plates, representing the appearance of meteoric stones from Stannern and Gross-

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* *Eine Leitform der Meteoriten von* W. HAIDINGER, 1860, *mit* 2 *Tafeln.*

Divina which are complete in themselves and may be considered as individuals of their kind, which at the same time show distinctly one of the periods through which they have pased.

In viewing meteorites we necessarily must start from certain fundamental considerations, proved by the phenomena themselves in order to arrive at a full understanding of their forms and conditions. These are: 1st, the stone leaves the extra-terrestrial space as a solid; 2d, its velocity is greatest on entering the atmosphere of our earth; 3d, it is retarded by the resistance of the air; 4th, the "fireball" (or luminous envelope of the meteor) is formed by the compression of the air and the rotation of the stone resulting therefrom; 5th, the termination of the first part of the path is marked by a detonation, the so-called "explosion," the vacuum inside of the fireball being suddenly filled by the surrounding air.

The crust is formed during the first part of the phenomenon by a rapid melting. The Stannern stone passed through the atmosphere with its rounded side foremost, which shows over its whole surface reticulated sulcations, resulting from the uniform action of the resisting atmosphere upon it, whilst the crust was in a viscous state. The lustrous crust is surrounded by a protruding gibbosity; the stone had formerly sharp edges, which however in the foremost direction of the meteorite were melted off and rounded and blown off towards the back part of the same.

The time of the passage through the atmosphere up to that of the detonation is very short, and generally lasts only a few seconds. The rising temperature, which produces the crust, belongs altogether to this period, since the stone came from the planetary space with a temperature of perhaps 100° C. below the freezing point. According to the greater or less conducting power of their constituents, meteorites will be heated more or less rapidly; masses of iron for instance may get red hot, whilst those, composed of bad conductors, remain cold inside, and, as soon as the influence of the heating ceases, or in other words, as soon as the detonation has taken place and the "fireball" disappeared, the inside and outside temperatures of the meteorite are soon counterbalanced and the crust is rapidly cooled, especially on account of the detonation taking place at a height, where the temperature is generally very low.

Masses like those of the Cape meteorites of 13th Oct. 1838, are probably the worst conductors of heat amongst the meteorites. They also have a crust very similar to that of Stannern, even in some spots the reticulated delineations on their "heads," but the crust is very superficial and the interior mass contains a considerable quantity of water of composition which can be expelled only at red heat.

During the real fall of the meteorite or after it already belongs to our planet, its velocity is, comparatively speaking, so inconsiderable, that a re-melting of its crust is improbable if not impossible.

From the form of the Stannern meteorite, the position of its crust and especially the protuberance, it becomes evident that the reticulated surface must have passed foremost during its passage through the atmosphere, and has formed the "head" of the same. Its centre of gravity, which always must be foremost in space, lies near that point. It could rotate on the axis of its path but could not be irregularly overturned. From its form we learn another fact; the stone did not burst in our atmosphere but it came from the planetary space of its present size, excepting that portion perhaps, which may have been melted off. It shows the result of a single uniformly progressive period in the formation of its surface and requires us to adopt a modification of our present views about the fall of meteorites. Certainly it is not always *one* meteor, which "explodes," and the fragments of which fall down as meteorites, but in many instances *whole swarms* of separate pieces enter our atmosphere, forming one common fireball, the bursting of which may be caused by rotation, and the separate detonations belong doubtless to the different individuals, brought to a stand-still.

The stone of Gross-Divina, which in its general character is allied to those of Timochin, Zebrak and Eichstädt, shows a great dissimilarity on its two principal planes, one being pretty smooth, the other very rough. The form of the whole stone is that of a fragment, altered only on its surface. Characteristic of this meteorite is a ridge, which passes over the "head" of it, and on account of which it had, whilst passing through the atmosphere with planetary velocity, alternately two different positions, first one plane foremost and then tilting over to the other. A large surface of nearly 80 square-inches compresses the air, and surrounds the cold stone with a "fireball," the flaming points of which, uniting immediately behind the stone, where we can imagine the greatest rarefaction of air, have the most favorable position to melt off the surface and produce the roundish spots resulting from their attack.

Corresponding with the ridge over the frontpart (or head) is one over the back part of this meteorite, as the boundary of the action of the two positions of it, produced by alternately tilting over from one side to the other on its cosmical path through the atmosphere. The roundish spots, where a melting off has commenced, have a striking resemblance to the impressions of fingers in dough, and have been frequently called "impressions" (Haidinger prefers the name "depressions"), and we can often learn the position of the stone during its cosmical course by a careful examination of their situation. We find them generally on that side of the meteorite best protected during its passage.

What is most remarkable in the foregoing is the fact that some of the meteorites have retained their equilibrium during the first period of their passage through the atmosphere.

[One fact in the physical history of meteorites seems not to be fully explained by the theory of Dir. Haidinger that the production of a crust appears impossible after the body reaches our atmosphere and explodes—namely that there is often a face of recent fracture plainly distinguishable from the older and more rounded faces by its general aspect and still more so by the extreme thinness of the crust over this newly exposed face. Such faces are visible on the New Concord (Guernsey County) Meteors of May 1st, 1860. It would seem to follow from Haidinger's reasoning that sometimes at least meteors should reach the earth having faces of fracture unglazed by any crust. Such in fact is the case in the *Quenggouk* meteor described on page 142. But this is a very rare if not an unexampled occurrence. We seem therefore driven to the conclusion that in the brief interval between the explosion and the body's coming to the earth (its *telluric period*) a new crust is sometimes formed over the freshly fractured surface.—s.]

3. *St. Denis-Westrem.**—At the meeting of the Imp. Acad. of Vienna of October 4th, 1860, Director Haidinger gives an account of the meteorite of St. Denis-Westrem.

The fall took place *without* detonation and accompanied only by slight noise, similar to the rattling of carriages, on June 7th, 1855, 7¾ P. M. near the town of St. Denis-Westrem, about 2½ miles from Ghent in Belgium. It fell about 30 paces from a man and woman. It penetrated the ground about two feet and was immediately dug up; it was still hot, blueish black and smelled sulphurous. It weighed 700·5 grammes, its sp. gr. = 3·293.

Its form is very remarkable, similar in shape to an "Ananchites," (a genus of Echinus,) a flat elongated base and an arched inclosure. It has the character of a real fragment and is incrusted all over. The crust on one side is very uneven, whilst the other portion is more even and equally rounded, especially on the edges. The edges between the rough surface and the rounded planes are well marked.

The centre of gravity foremost indicates the direction of the motion, which is also confirmed by the rounded, though not very marked, depressions on the opposite side. The position may indicate that the rotation has been exceedingly influential in rounding it by melting off the outermost inclosure. The fusibility, however, must be very trifling, the crust being very thin and of very little lustre.

The absence of a detonation in this case as well as at Linum near Fehrbellin (Sept. 5th, 1854), only represented by some rattling, blustering or hissing, when compared with the tremendous sounds and roaring, such as occurred at New Concord for instance, is worthy of our fullest attention. The size of the meteorites seems to have nothing to do with it. Every one has an independent motion through space, until, after being exposed to a series of various conditions within a space filled in its centre by our earth, it finally becomes a part of the same. The earth itself has a rapid motion round the sun, every point of its surface one corresponding with the daily rotation round its own axis. It certainly can occur that a meteorite rushes from space tangentially upon the orbit of the earth with a velocity of little more or less of 20·5 miles in a second, in which case the meteorite overtakes the earth or is overtaken in its course thereby, and this at such a distance that it is then really attracted by it and must finally reach it according to the laws of gravitation. It might be possible in an extreme case that on account of the absence of the compression of air, no crust would be formed. But this being present both on this meteorite and that of Linum, there may have been a lesser degree of compression of the air but not a total absence of it. At the same time, however, the resistance in the rotation of the atmosphere may have its influence, which can act in opposition to the rotation of the meteorite round its own axis.

The stone resembles those of Reichenbach's 2d family, "somewhat blueish stones," Slobodka, Château-Renard, Lissa, etc., and especially that of New Concord, Ohio.

The stone contains finely disseminated iron and pyrrhotine, the latter sometimes filling up vein fissures, which give it the character of a fragment from a very large mass, a real mountain of rock.

Disseminated through the whole mass are spots of so called ironrust and globules, somewhat crystalline in their cross-fracture, which leave impressions when falling out of the brittle mass.

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* *Der Meteorite von St. Denis-Westrem im k. k. Hof. Mineralien-Cabinete von* W. Haidinger. 1860 (*Oct.*).

4. *Calcutta Meteorites.**—At the meetings of the Imp. Acad. of Vienna of June 8th, November 3d, and the last one of the year 1860, W. Haidinger has given some accounts of the "Calcutta Meteorites," which had been acquired a short time before by the Imperial cabinet of minerals, of which we give the following abstracts.

(1.) The Meteorite of *Shalka* (or Shaluka) in Bancoorah fell about 80 yards south of the village in a rice-field on the 30th Nov., 1850, three hours before sunrise. The fall was witnessed by two persons. The noise, compared with thunder, although peculiar in its nature and by the natives called "gurgur," "charchar purpur," was hardly very loud, because it did not wake up the inhabitants of Bhora ¾ of a mile distant. The stone had penetrated the ground to a depth of about 4 feet, by digging for it fragments were found to the depth of three feet and in a circle of about 20 feet radius. It appears that only *one* stone fell, which, however, was broken into fragments by striking the ground, and it may have been three feet long. It came from a southerly direction at an angle of about 80°. It must have been hot, when it fell, the black crust here and there peeled off and stuck to the earth in small fragments; the surrounding earth had the appearance of burnt clay.

The stone is very peculiar, the fine grained portion of a whitish color resembles pumice, the darker ash-colored crystalline portion forming coarsely grained aggregations of individuals of two lines in every direction, have the appearance of pearlstone, the latter forming globular masses of several inches in diameter, inclosed in the former or penetrated by it in a veinlike manner, the whole mass having the appearance of a breccia. It is friable like coccolite. The larger individuals show a distinct cleavage in one direction, less so in another, intersecting at angles of 100° and 80°; perpendicular upon them terminations are sometimes met with, which can be considered as planes of crystals and parallel of which no cleavage is observable; twin composition parallel with one of the prismatic planes. The *real* fracture shows greasy lustre. The mass is extremely friable and fragile; H. =6·5; Sp. gr. at 19° R. =3·412.

Small grains up to the size of a millet-seed of chromic iron, sometimes in octahedral crystals are intermixed with the ash-colored mass. The dull blackish brown crust is not thicker than writing paper; here and there it forms irregular reticulated particles, which show somewhat more lustre.

It does not contain any metallic iron. The grey mass contained according to K. von Hauer:

Silicic acid,	=57·66	....	=30·50
Alumina,	=*trace*	....	....
Oxyd of Iron,	=20·65	=4·58	=12·61
Lime,	= 1·53	=0·43	
Magnesia,	=19·00	=7·60	

and stands in the middle between the bi- and tri-silicates of monoxyds, the oxygen ratio being =1 : 2·42. It is named by Haidinger in honor to the late Henry Piddington, and resembles in composition an olivine-like mineral, (alleged to have come from the meteoric iron of Grimma) and Shepard's Chladnite.

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* Die Calcutta-Meteorite, von Shalka, Futtehpore, Pegu, Assam, und Segowlee im k. k. Hof Mineralien-Cabinete von W. Haidinger.

The Shalka meteorite belongs to Reichenbach's first family (?) first group and Shepard's Chladnitic-trachytic stones.

(2.) An interesting fall of meteorites occurred on December 27th, 1857, 25 minutes after 2 A. M., at *Quenggouk* near *Bassein* in Pegu. Three stones, which were evidently fragments of *one* meteorite, we found five and ten miles apart. According to the information, which Haidinger received from Mr. Oldham, the meteor, which emitted the stones had the appearance of a large inverted umbrella in flames, as observed 90 miles south of Quenggouk on the river Bassein, passing from W. to E. at an altitude of 40° to 50°, and after the termination of its path giving a report like that of a monster gun, followed by a rumbling noise. Another observation made on board of the steam-frigate Semiramis about 200 miles S.E. from where it fell, describes it as having had at first the appearance of a large star, rapidly increasing in size until it was about three times as large as the moon, leaving behind a long tail and falling toward the east.

From these observations Haidinger made the following deductions: 1st, the height of the meteor was from 80 to 120 miles (16 bis 24 meilen); 2d, the original cosmical course was E.N.E. to W.S.W., the latter part of it, however, from W. to E. this change being caused by the resistance of the atmosphere; 3d, the size of the meteor was, if the word *three times* is referred to the area of the moon, 14,400 feet, if to its diameter, 24,000 feet; 4th, the pieces found between five and ten miles were thrown asunder by a real explosion, produced by the rotation of the meteor; 5th, the pieces were real fragments, which fitted together and the fracture planes of which were not covered by a crust, a proof that the telluric fall did not take place with cosmic velocity; 6th, the detonation was heard at 100 miles distance but not at 200 miles.

(3.) Another fall, on which Mr. Oldham reported to Haidinger, took place at $2^h\ 14^m$ P. M., on July 14th, 1860, at *Dhurmsala in Punjab*, and was accompanied by a tremendous noise and a great number of detonations, similar to those of the discharge of heavy artillery. The earth was shaken in convulsions and trembled. Three witnesses saw a flame of two feet broad and nine feet long, passing obliquely over the station after the first explosion had already occurred. The direction is stated as N.N.W. to S.S.E. and stones were found at five different places in this direction; it is said that stones had also fallen at several other localities.

They penetrated the ground to a depth of from 1 to 1½ feet, and it is reported that persons, who picked up some fragments, before they held them in their hands half a minute, dropped them again, owing to the intensity of cold which quite benumbed their fingers. The largest piece found weighed 320 lbs. The observation of the intensity of cold of the fragments is a matter of great interest and importance. The little meteorite passes in its cosmical path during an incalculable period through an excessively cold space. By the resistance of the atmosphere, light and heat are developed, which latter might even produce a melting of the surface of bad conductors, whilst the inside of it remains quite cold.

(4.) The fall of the meteorites at *Futtehpore*, Nov. 30th, 1822, has already been mentioned by Prof. Shepard (this Journal, 2d series, xi, 36). The mass is pale ash-grey, fine-grained, and on the fructure planes it shows yellowish brown spots of so-called iron-rust; vein-like plates of pyrites of a reddish bronze yellow color pass through the mass; the polished surface shows many particles of metallic iron, the largest observed grain was 1½ lines long and one line broad. The mass is crossed by fissures in different directions intersecting at well defined angles, some of them filled up with the material resembling that forming the crust, others by magnetic pyrites and iron. These fissures show the phenomena peculiar to veins, they intersect, join and throw each other out of their course. Although the mass is soft and may be scraped easily, it contains here and there harder globules of a greyish white color, others have a darker grey color, and iron is accumulating in or around them. Some of the inclosed fragments are angular and not round in their intersections, others have a plate-like linear structure and are lustrous. The crust is dull, brownish-black, here and there, with isolated or groups of roundish flat impressions; the surface of the crust has the appearance, as if it was made up of angular plates of an irregular form of 2–3 lines in diameter. It is less than half a line in thickness and contains particles of metallic iron. Haidinger found the sp. gr. at 17° R. =3·526.

It belongs to Reichenbach's whitish meteorites, 2d family, 1st group.

(5.) The stone of *Pegu* was found in 1854, and appears to have fallen but a short time previously. The mass is pale gray a little bluish, made up of round grains or granules, imbedded as it were, in white sand, slightly coherent and almost friable. The grains are both round and angular and from greyish white to dark smoke-grey, the largest not over one line in diameter. Exceedingly small particles of met. iron and pyrites (or pyrrhotine) are pretty regularly disseminated through the mass. Very interesting in the examined specimen is a real vein apparently of pyrrhotine of about half a line in width, a positive proof of its subsequent formation in a large mass.

The crust is greyish black inclining to brown, without lustre and not over ¼ line in thickness. Sp. grav. =3·737. It belongs to Reichenbach's second group.

(6.) The real locality of a stone, which was found in 1846 and which Piddington supposes to be from *Assam*, is not known. Two of the three procured pieces were fragments of the same stone, the other appears to come from a different one as the proportions of nickel and cobalt are not the same. It is beautifully marbled, very solid and compact and admits of a good polish. It resembles the meteorites of Seres, Barbotan, Mezö-Madaras, l'Aigle and others of the 3d family of Reichenbach. The mass is dark grey, irregular rounded fragments of a paler color and nearly half an inch in diameter are imbedded in it, also numerous more or less globular particles, small quite black ones and smaller and larger ones of a bright ash grey color. Inclosed in the pale grey colored portions are brown, also white and yellow metallic fragments. Disseminated in every direction through the paler and darker portions are about equal quantities of metallic iron and pyrrhotine, the distribution of a portion of the iron particles as fringes around the inclosed spherules is very remarkable. It is a proof that during the gradual solidification, after the globules had already been inclosed, the iron particles were yet capable of locomotion.

The crust is dark greyish black and has here and there some lustre. It is very thin and shows some round flat impressions, indicating the back part of the meteorite. Sp. gr. at 17° R. =3·792.

(7.) The fall of the *Segowlee* meteorite took place on March 6th, 1853 at noon. Persons at a distance of a few miles from the place of its fall heard a peculiar rumbling noise very dissimilar to thunder, more like the rattling of carriages over a paved street. A man and a boy heard something heavy fall without any other noise and collected some of the stones. Afterwards others were picked up and about 30 were found scattered over an area of about one square mile. All the stones were nearly pyramidal and weighed from ½ to 4 pounds, one however 14½ pounds. The stone, which Haidinger describes is very unlike any in the Imperial collection, but resembles in color that of Mentz (Mainz). The brown mass is very solid but by no means homogeneous, imbedded in it are numerous round and angular paler and darker particles of more or less hardness, and disseminated through the whole mass fine particles of metallic iron as well as pyrrhotine, more of the latter, the largest grain being two lines long and one broad, the largest grain of the iron ¼ of that size. Many fissures pass through the mass, without any regularity, however. It has a dull appearance, like iron covered with rust, and the whole looks like a poor variety of brown iron ore. A real fresh fracture shows distinct, although slight greasy lustre. The crust is very thin, not over ¼ line in thickness, dark reddish brown mostly dull, only here and there on the flat portions and the rounded edges darker inclining to black and somewhat more lustrous. The whole condition gives the proof of a very slight fusibility. Sp. gr. at 17° R. =3·425. Hardness about 6.

The form of the large stone is very remarkable. Its centre of gravity lies evidently in its thicker part, at the opposite lighter and pointed end are principally the indications of those flat, basin-like depressions. The lower part of the meteorite, as it were the base of it, is remarkably even.

5. *The meteoric iron from Tula, Russia.*.—In the year 1846 a mass of iron of over 15 puds (542 pounds avoirdupois) was found 7 versts (4·6 miles) from the station Mariinskoje (in the Government of Tula) on the Moskaw-Tula Road (54° 35′ N. lat. and 37° 34′ E. of Greenw.). It was sold for four roubles (three dollars) to the Myschega iron works in the neighborhood, where the greater portion of it was worked up into axles, wagonsprings, grapnels and other implements, until in 1857 Dr. Auerbach learned of its existence and saved the remainder from destruction. He has given the first notice of it,* and in a preliminary analysis of it found the mass to contain: Iron =93·5, Nickel =2·5, traces of Tin and 0·9 Schreibersite. It yields sulphydric acid, when dissolved in chlorhydric acid, probably owing to the presence of pyrrhotine. Articles manufactured from this iron show after etching a beautifully damasked surface.

At the meeting of the Imperial Academy at Vienna of Nov. 18th, 1860, W. Haidinger has communicated some very interesting observations made on a specimen of this iron obtained from Dr. Auerbach.

The principal mass, although not altogether homogeneous, consists of *iron*, showing indications of Widmannstädtean figures, and imbedded in it pieces of *meteoric stones* with a large proportion of metallic iron disseminated through the same. The angular shape of the pieces and the irregularity of their borders leave not the least doubt as to their nature. They are *real fragments*, separated from larger masses by mechanical force. The uniformity of the fine-grained mixture of the fragments as well as that of the completely metallic nature of the inclosing mass corroborate this opinion. The spec. grav. of a piece of the fine-grained stone at 12° R. was found to be 4·153; that of the iron as 7·332.

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* Bull. Soc. Imp. de Moscou, 1858, No. 1, page 331.

Even before etching, but far more plainly afterwards, three objects can be perceived on the surface of this iron. The greater portion of it is acted upon by the acids, but projecting over it are figures in full lustre, partly angular, partly made up of nearly parallel lines; the length of these ridges is at most four lines, the width of the intervals between two is from one-tenth of one line up to one line, the width of the ridges themselves is hardly one-twentieth of a line. They are evidently Widmannstädtean figures, although they have not the regular arrangement of those of Agram, Elbogen, Lenarto, Toluca, Durango. Similar to those of Bohumilitz the particles of Schreibersite are distributed in ridges along the separating lines of the individuals themselves, they even inclose granular particles made up of very minute individuals. This being the case is shown by the damask-like, although faint lustre of these planes. Besides the fine Schriebersite lines, the etching developes a brownish black lustrous silicate, sprinkled over the metallic surface like fine sands or in little fragments.

The Widmannstädtean figures of the Tula iron show a striking resemblance to those of that of Burlington, Otsego County, N. Y., the latter however is far purer and especially without the fine sandy silicate.

The larger fragments of the enclosed meteoric stone are allied to those containing a large quantity of metallic iron, as for instance to that of Tabor, although the mass is of a dark brownish grey, almost a brownish black color; in the size of the grain it resembles the stones of Charsonville, although the greenish grey color of the latter is far more light.

Judging from analogies observed upon our earth, especially from the appearance of the perfectly angular and not in the least worn off fragments of meteoric stones imbedded in the iron, Haidinger comes to the conclusions that, before the stony masses were inclosed in the iron, they were united as portions of real rocks in *one and the same heavenly body*, from which they came to our earth; and that the metallic *nickeliferous iron formed veins in the granular rock*, which latter was a mixture of metallic iron and a silicate of iron and magnesia before being broken off its connection, an instant which can be considered the starting point, at which the meteorite was set in motion in the cosmical path the conclusion of which is its arrival upon our earth.

The forms of the larger and smaller lumps (cloddy masses?) inclosed in meteorites show, however, many peculiarities, which require a more thorough investigation.

In the meteorite of Hainholz are imbedded globular and ellipsoidal lumps of iron of the size of a hazelnut. They are *no fragments*, and contain, as pointed out by Baron von Reichenbach, who described them, smaller globules or clods of sulphid of iron, the latter trimmed with Schreibersite. It shows these iron lumps not in a very conspicuous manner, they are completely grown together and interlaced with the surrounding silicate. The mass shows after etching a very peculiar appearance, the iron appears in small lumps of about two lines in size of a perfectly homogenous structure, visible by simultaneous reflection, but the interior of these are dendritically marbled by inclosed particles of silicate. Besides, there are larger somewhat round particles of the latter and smaller angular ones of one half to one line in diameter, and very remarkable and characteristic pretty large highly crystalline olivines with distinct cleavage. However crystalline the latter may be, they nevertheless have a very irregular surface. Beyond doubt they have been formed as crystals but since their formation have lost their external form.

To consider these iron lumps as well as the olivine crystals and fragments, as Baron von Reichenbach does, as belonging to a previous formation of meteorites or heavenly bodies in a distant space of the universe, is certainly not requisite; in their habitus they have a striking resemblance to the more or less compact formations of basaltic and trachytic tufa, of course not taking into consideration the action of water or carbonate of lime.

Haidinger suggests to consider the structure of a great number of the known meteorites as dry tufas, formed without water, *a meteoric tufa*, an idea which as he remarks may be the starting point of a long series of inferences leading us far away into the field of suppositions of an earlier original formation, but always without losing sight of their immediate connections.

6. *The meteoric iron from Nebraska.*—W. Haidinger gave at the meeting of the Imp. Acad. of Vienna of Dec. 13, 1860, some information regarding the meteoric iron from Nebraska, obtained by Mr. Nathaniel Holmes of St. Louis, Mo.

The original mass weighed 35 pounds and was found on the right shore of the Missouri River in Nebraska Territory, 25 miles west of Fort Pierre, 44° 19′ lat., and 100° 26′ W. of Greenw., whence it was brought in 1857 and presented to the Academy of St. Louis in 1858. It weighed 30½ pounds and the surface was hardly acted upon by rust. According to an analysis by Dr. H. A. Prout, it contains:

Iron,	=94·288
Nickel,	7·185
Magnesium,	0·650
Calcium,	0·350
Sulphur,	*trace*
	102·473

but not the least trace of cobalt, chrome, manganese or any other constituent. (The Widmannstädtean figures, however, would indicate the presence of phosphid of iron and nickel.)

A segment of the Vienna specimen cut nearly parallel with an octahedral plane showed striæ of half a line in width intersecting at angles of 60° and 120°, with the triangular and rhombic intervals between the inclosing ledges of Schreibersite covering the whole etched surface. They indicate undoubtedly a long continued activity of crystallization during an immeasureable period of time. The Widmannstädtean figures show the closest resemblance to those of the Red River iron preserved in the Yale College Cabinet.

A lithographic plate given in the 4th number of the 1st vol. of the Transactions of the St. Louis Academy represents the whole mass and, on account of the numerous flat depressions, which it shows, evidently from the reverse of that side, which passed foremost through the atmosphere. (See above, Haidinger's views on the typical forms of meteorites.)

F. A. Gth.

7. *Abstract of Meteorological Observations, made during the year* 1860, *with the averages of eight years, at Sacramento, Cal.*, lat. 38° 31′ 41″, lon. 121° 29′ 44″. Altitude at the top of the levee, in front of the city, 54 feet 5 inches. Height of lower surface of the mercury, 52 feet 5 inches above low tide at San Francisco—with remarks; by THOMAS M. LOGAN, M.D.

1860.	January.	Feb'y.	March.	April.	May.	June.	July.	August	Sept.	October	Nov.	Dec.	Ann. Mean.	8 y'rs av'g
Barometer.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
Maxima,	30·366	30·599	30·334	30·331	30·280	30·103	30·115	29·967	30·058	30·248	30·357	30·234	30·249	30·256
Minima,	29·797	29·790	29·786	29·728	29·723	29·725	29·684	29·650	29·751	29·769	29·753	29·740	29·825	29·722
Mean,	30·145	30·141	30·047	30·017	30·000	29·886	29·909	29·808	29·911	29·990	30·074	30·020	29·995	30·003
Thermometer.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.
Maxima,	56·00	65·00	64·00	77·00	70·00	84·00	87·00	90·00	87·00	78·00	65·00	59·00	73·50	77·54
Minima,	37·00	37·00	39·00	44·00	46·00	58·00	61·00	62·00	57·00	47·00	40·00	38·00	47·17	46·30
Mean,	46·20	49·83	53·30	57·82	38·48	65·64	73·17	73·50	67·59	59·76	53·47	49·34	59·01	59·90
Thermometrograph.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.
Maxima,	56·00	66·00	65·00	78·00	74·00	86·00	93·00	92·00	89·00	81·00	67·00	60·00	75·58	78·00
Minima,	32·00	32·00	34·00	38·00	37·00	48·00	51·00	54·00	50·00	41·00	35·00	33·00	40·42	41·58
Range,	24·00	34·00	31·00	40·00	37·00	38·00	42·00	38·00	39·00	40·00	32·00	27·00	35·16	36·42
Force of Vapor.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
Maxima,	·405	·376	·439	·519	·483	·547	·637	·651	·591	·559	·410	·423	·503	·521
Minima,	·178	·097	·173	·206	·191	·305	·356	·397	·283	·225	·140	·182	·228	·205
Mean,	·262	·258	·316	·342	·358	·421	·476	·478	·411	·372	·266	·283	·354	·357
Relative humidity.	Per cent	Per cent	Per cent	Per cent	Per cent	Per cent	Per cent	Per cent	Per cent	Per cent	Per cent	Per cent	Per cent.	Per cent
Maxima,	94·00	92·00	93·00	93·00	87·00	83·00	78·00	75·00	77·00	88·00	86·00	88·00	86·17	87·59
Minima,	54·00	28·00	53·00	28·00	41·00	32·00	38·00	33·00	25·00	39·00	28·00	58·00	38·08	32·84
Mean,	82·86	72·10	76·90	71·97	72·65	67·13	64·00	58·89	61·91	72·76	65·34	78·89	70·45	69·92
Number of days clear,	13⅔	8⅔	8	12⅓	11	19⅓	23⅓	23⅓	20	7⅔	12	6	165⅓	193⅓
Number of cloudy days,	17⅓	20⅓	23	17⅔	20	10⅔	7⅔	7⅔	10	23⅓	18	25	200⅓	171⅔
Number of rainy days,	6	8	14	8	10	2	3	0	2	8	2	17	80⅔	60⅔
Quantity of clouds,	3·5	3·4	4·5	2·8	3·9	1·3	0·7	0·4	0·8	3·7	2·1	5·1	2·7	3·5
Quantity of rain and fog,	2·310	0·931	5·110	2·874	2·491	0·017	0·549	0·000	0·063	0·914	0·181	4·282	19·722	17·372

1860.	January.		Feb'y.		March.		April.		May.		June.		July.		August		Sept.		October		Nov.		Dec.		Ann. Mean.		8 y'rs av'g	
1st dys & 2d–Force N. wind	12⅓	2·0	11⅔	2·3	9⅓	2·1	7⅔	1·7	3	1·2	1⅔	0·8	1⅓	0·7	2	0·9	2⅔	1·3	10	1·6	15⅔	1·9	9	1·2	86⅓	1·5	52⅔	1·5
" " " N.E. "	2⅔	1·3	1⅓	0·9	2	1·2	2	1·2	⅔	0·7	0	0·0	⅓	0·3	1	0·4	1	0·3	0	0·0	1	1·0	2	2·0	13⅔	0·8	14	1·1
" " " E. "	2⅔	1·8	2⅔	1·8	1⅓	1·1	0	0·0	⅔	0·7	⅓	0·3	⅓	0·3	⅔	0·3	0	0·0	⅔	0·5	⅓	0·3	2	1·2	11⅔	0·7	12⅔	1·1
" " " S.E. "	5⅓	1·7	2⅔	1·1	5⅔	2·0	5⅓	2·5	3⅔	1·7	⅔	1·3	1	0·3	4⅓	1·4	4	1·1	3⅔	2·1	5	1·5	6⅓	2·6	47⅔	1·6	72⅔	1·8
" " " S. "	3⅔	2·1	3⅔	2·1	6⅔	2·5	5	2·3	8⅔	2·1	15⅔	2·2	15⅓	1·8	11	1·9	8	2·3	6⅓	1·7	3⅓	2·0	8⅓	2·1	95⅔	2·0	67	2·1
" " " S.W. "	⅔	1·3	2	1·4	1⅔	1·7	4⅓	2·4	5	2·2	5	2·5	6⅔	1·7	6⅓	1·9	5	1·2	4⅔	1·7	1	1·3	1	2·0	43⅓	1·8	57⅓	1·9
" " " W. "	⅓	1·0	1⅓	1·1	3	1·8	2	1·9	8	1·8	5	1·6	5⅓	1·6	4	0·9	3⅔	0·9	2⅔	2·1	1⅓	1·0	2	2·4	38⅔	1·5	18⅔	1·2
" " " N.W. "	3⅔	2·1	3⅔	2·6	1⅓	2·3	3⅔	1·4	1⅓	0·9	1⅔	0·9	⅔	0·7	1⅔	0·4	5⅔	1·4	3	1·7	2⅓	2·0	⅓	2·0	29	1·5	71	1·9

Disturbances of the electrical equilibrium are rarely manifested in California, and but four times have lightning and thunder been witnessed in the whole year, 1860, viz: on May 3d, July 11th, September 26th and October 17th. During the storm of September 26th, which was accompanied with a sprinkle of hail, the lightning was very vivid, preventing for the first time since they have been set up, the working of the telegraph wires. Hail and snow are also seldom experienced. Besides the instance of hail just mentioned, we were visited with a sprinkle of both hail and snow on the 3d of May.

The Aurora Borealis, which has been seen here only eleven times in the same number of years, has been witnessed on no less than four different occasions during the last year. The first and most remarkable was that on the 4th of July, and although the brilliancy was not so great, nor the field of observation so extended as that on the 28th of August of the preceding year, still it was seen at places wide apart, and at each point of observation presented the same distinctive features. This is at least the second instance on record in which this phenomena has been observed cotemporaneously in California and the Eastern States.

Earthquakes have not been as frequent during the year at San Francisco as usual, and the only one recorded, in our Register here, was not felt in the former place, nor at any other point among the Coast Range mountains. This, at Sacramento, was experienced on the 15th of March, at ten minutes past 11 h. A. M., during a stagnant and cloudy condition of the atmosphere. The direction of the impulse seemed to be from northeast to southwest—as it was sensibly experienced at various places between here and Carson City. At the latter place (4,741 feet above the sea), the intensity of the force was considerably greater than at Sacramento, where it was only sufficient to cause a slight vibratory motion among chandeliers and other pendant objects.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *National Observatory.*—The accession of Lt. JAMES M. GILLISS to the direction of the Washington Observatory is a fact of happy omen for science. It may perhaps seem to some but a tardy act of justice that the man who was chiefly instrumental in securing the establishment of this Observatory should at last be placed at its head. But whatever there is to regret in the past, (and alas how much there is!) in the administration of this Institution, it is a matter of general congratulation in scientific circles that the distinguished chief of the Chilian Astronomical Expedition should now adorn the post lately so ignominiously left vacant.

2. *Earthquake at Mendoza.*—On the 20th of March last, about 8½ o'clock in the evening, a most destructive earthquake overwhelmed the city of Mendoza in the Argentine Republic, South America. Mendoza is in lat. 32° 53′ S., lon. 69° 6′ W., and is about 2900 feet above the sea on the eastern slope of the Andes. The shock is said to have come from the N., followed by another from the S., and to have lasted only about five seconds, in which brief time nearly the whole town was utterly laid waste and from 8000 to 12,000 lives were destroyed. The shock was not felt at Valparaiso, distant about 150 miles in a right line W. by S.

3. *Prussian Expedition to Japan and China.*—A letter from Baron Richthofen, Geologist to the Expedition, from Yeddo, dated 25th of December, 1860, states that they were about leaving Japan, to go to Shang-

hai and then to the Phillipines, Siam and Java. The expedition thence returns to Europe by sea while Baron Richthofen proposes to cross the Asiatic continent.

4. *Correction.*—A letter from Prof. James Hall, in regard to trilobites from the Wisconsin 'Potsdam,' says: "The specimen you send me is from *Trempeleau* and not Black River Falls, as I had anticipated—having made a journey of thirty miles over bad country to "Black River Falls" to find the rocks there of *Sienite* with a little coarse grey sandstone a mile below." "The Lingula accompanying the trilobites is *L. polita*, (see Geol. Rep. Wisconsin, 1860.)" I believe the original authority for the locality was Prof. Daniels. F. H. Bradley.

Book Notices.—

1. *Ancient Works in Western New York.*—In the 13th Annual Report of the Regents of the University of the State of New York we find, among other matters interesting to science, an important document on the remains of early human art in the region contiguous to Lake Erie, and comprising the counties of Chautauque and Cattaraugus—by T. Apoleon Cheney, *Civil Engineer*, &c., 1859.

In a cursory perusal of this report and after an inspection of its illustrations, we are convinced that the dominion of that very ancient and unknown people, by whom the earth works of North America were raised, extended to Lake Erie and was a part of the great system of similar works which occupied the territory of several of the Western and Southwestern States, to a great extent, also, the valleys of the Ohio and Mississippi and their confluents extending quite to Mexico. There is so great a similarity, not to say identity, of structure, position, arrangement and contents whether of human remains, or of artistic works, when art was young, or of collections of minerals and other objects of nature and art that our curiosity is excited still more to obtain a solution of the question, 'who were the people who erected these remarkable works, forts, lines of defense, mounds of observation, or barrows of sepulture?' Our modern Indians have no such skill—nor have we reason to believe that any such structures as are named above were ever erected by them or their ancestors and this appears to be the opinion of Mr. Cheney.

His labors have been praiseworthy—himself an amateur archeologist, he works, we believe, without pecuniary reward, deriving no substantial aid from his investigations, and if we are not misinformed, under the pressure of ill health and with a family entirely dependent upon his exertions in other branches of research and literature—and who have at times suffered in an extreme degree.

We sincerely wish him every aid to which he is so well entitled—especially to enable him to bring a successful result extensive investigations in which he is now engaged as to the origin and history of the builders of the mounds and fortresses which are the only record of that wonderful extinct people.—B. S.

2. *An Elementary Treatise on Human Anatomy;* by Joseph Leidy, M.D., with 392 illustrations. Philadelphia: Lippencott & Co. 1861. 8vo, pp. 663.—This original and truly admirable treatise on Human Anatomy bears everywhere the signs of its author's genius. Concise and clear without formality, learned without pedantry, it is a model of what every text book in a descriptive science should be. The illustrations are excellent and mostly from original drawings of the author or his friend

Dr. Schmidt, put on wood by the masterly hand of August Wilhelm, a young artist of Philadelphia. Every mechanical detail of paper and typography has received scrupulous care.

3. *Handbuch der metallurgischen Hüttenkunde;* von Bruno Kerl, 2te, *umgearbeitete und vervollständigte Auflage.* 1er *Band*, 1te *Abtheilung*, Freiberg, 1861.—This first instalment of a second edition of Prof. Kerl's excellent and exhaustive treatise upon metallurgy, revised and brought up to the present date, will be welcome to every one who takes interest either in the theory or practice of smelting.

To those familiar with the previous edition of this work we need not urge its encyclopædic character or its importance to practical men. Certain it is that no other handbook upon the subject can be mentioned in comparison with it. In breadth of scope, amplitude and completeness of detail, and honesty of purpose, the work is on a par with any of the standard treatises upon other branches which do so much credit to the scientific literature of Germany. Though comparatively little has been heard heretofore of the scientific merits of this work, it nevertheless contains in reality,—incidentally as it were,—almost everything which is known of the chemistry of the metals, while the scrupulous care with which references to original memoirs, etc., are given, and the abundance of these citations renders it an invaluable aid to the student and investigator. The book is copiously illustrated and well printed upon excellent paper in the best style of the house of Engelhardt (Thierbach.) Westermann & Co., of New York have it in store. F. H. S.

Obituary.—

Dr. Charles Robb, Professor of Natural Science at the University of New Brunswick, died at Fredericton, April 2, aged 46. He received his medical degree at the University of Edinburgh, and having subsequently studied at Paris entered on the duties of his professorship in 1827, which he filled for twenty-four years with zeal, fidelity and ability. Although residing at such a distance from any scientific centre and without the means or opportunities for original research he was nevertheless known to his associates and friends as an accurate observer and deep thinker. He made a careful study of the geology and mineral resources of New Brunswick and the museum of his College which he collected and arranged, remains the result of his labors. In all matters connected with the advancement and prosperity of the Province he took a deep interest. He was for many years Secretary to the Provincial Board of Agriculture and he will long be remembered as one of the pioneers of science in New Brunswick.

Hermann Dauber, the successor of the lamented *Grailich*, as adjunct-curator of the Imperial Austrian Mineral Cabinet, died at Vienna, March 12th, in the 38th year of his age. Dauber's well known contributions to Crystallography, during the past fifteen years, have placed him in the front rank as a crystallographer and will cause his name long to be remembered as one of the most accurate and conscientious observers in this department of science.

Books Received.—

De Schlagintweit's India and High Asia. Text. Vol. I, Astronomical and Magnetic Observations. 4to, pp. 494. Leipzig: F. A. Brockhaus.

Recherches Expérimentals sur l'Élasticité des Méteaux faites à l'Observatoire physique centrale de Russie par A. T. Kupffer (Directeur, &c.). Tome I (avec 9 planches). St.-Pétersbourg. 1860. 4to, pp. 430.

Annales de l'Observatoire physique central de Russie publiée par ordre de sa Majesté Impériale par A. T. Kupffer, Anne 1857. Nos. 1 and 2.—St.-Pétersbourg, 1860. 494 and 111.

Compte-Rendu Annuel adressé a S. Exc. M. de Knajévitch, Ministre des Finances, par le Directeur de l'Observatoire Physique Centrale A. T. Kupffer. Année 1858. St.-Pétersbourg, 1860. 4to, pp. 51.

Lebenslinien der Meteorologischen Stationen am Kaukasus, von A. Moritz. St. Petersburg, 1859. 4to, pp. 12.

Mémoires de la Société de Physique et D'Histoire Naturelle de Genève. Tome XV. 2me partie. Genève, 1860. 4to, pp. 200-525.

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[*Continued in next Number.*]

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XVI.—*Waterglass;* by J. M. ORDWAY.

PART I.—HISTORY AND MANUFACTURE.

THE soluble alkaline silicates have of late years come into extensive use in the arts, and seem likely to constitute a large and permanent branch of chemical manufactures. The common works on technology do not treat of them as fully as their present importance demands, nor can all that could be desired be found within the limits of any single paper hitherto published on the subject.* It is here proposed therefore to collect details from various scattered memoirs, as well as from private experience, and present a concise but more complete account of the history, manufacture, nature, and uses of the soluble silicates of potash and soda.

The later alchemists were acquainted with the deliquescent tersilicate of potash,—$\dot{K}_3\dddot{S}i$,—the basis of *liquor silicum.* Thus Glauber, in giving his second method of testing sand for gold, says:—"Take one part of white quartz or sand, mix it with three or four parts of salt of tartar, or any other alkali, and put the mixture into a crucible, but so as not to fill more than one-third of it; since otherwise, in melting, the mixture would rise and flow out of the crucible. Let it stand [in the fire] half an hour that it may be well ignited and changed to a pellucid glass.

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* A pretty full resumé of all that had been published on Waterglass up to 1857, is given by E. Kopp in the Moniteur Scientifique, tome i, livraison 4me.

Pour it out and dissolve in water, or better, in lye; and the sand or silex will be dissolved and changed into a thick water."*

In 1825 there appeared a memoir in German "On a New and Useful Product Obtained from Silex and Potash, by Dr. Johann Nepomuk von Fuchs," in which we find the following account of the first discovery of soluble glass, or, as Fuchs provisionally named it, *waterglass*:—"I obtained it first about seven years ago, in pouring concentrated potash lye on very finely divided silica which had been precipitated from *liquor silicum* with salammoniac, and well dried. The potash was absorbed by the silica with a sensible elevation of temperature, and the whole soon changed into a very firm, transparent, glass-like mass which proved to be permanent in the air. It did not occur to me that the same thing might also be produced by dissolving silica in potash and evaporating the fluid; since I then, and for a long time afterwards, held with all chemists the erroneous notion that a combination of silica and potash, to be permanent in the air, must be insoluble in water, and that a soluble product must necessarily be deliquescent. Two years afterwards, when I wished one day, for analytical purposes,—for which I first brought silicate of potash into use,—to prepare some as fully saturated with silica as possible, I learned to procure the substance under consideration, by the method of solution. For this purpose I took freshly precipitated silica, poured on it as much potash lye as I judged to be necessary for its solution, and brought it to a boil. The silica very soon disappeared, and, in order to saturate fully the alkali present, to my no slight astonishment, I was obliged to add a quantity of silica still greater than I had taken at first. After this was done the solution was boiled a long time to concentrate it, and thus came to the consistency of syrup; and on the surface there appeared a tough pellicle which dried in the air to a transparent glass. All bodies which came in contact with this fluid received a glassy covering that attracted no moisture from the air but became much the harder and more brittle. From this I saw that the product before me was the same which had been obtained before by the process of absorption."

The author goes on to say that the burning down of the theatre at Munich contributed to the completion of his discovery. For when the new building was to be erected, diligent inquiry was made for something to protect wood against fire; and many substances having been tested and rejected, it occurred to him to make trial of the waterglass. Therefore with Dr. Pettenkofer as an associate, he made many experiments which showed its entire efficiency as a protective agent, and it soon assumed a greater importance than he had at first attached to it. They then labored to find some cheap and easy mode of preparation, which might render waterglass available for economical uses. In the first place they made *liquor silicum,* and dissolved in one portion the well washed silica precipitated from another portion. This method was too costly, and the product was not all that could be desired, since it contained too much carbonate of potash. Finally by trying the direct fusion of various mixtures of potash, sand, and charcoal, they got a product which, when powdered, dissolved slowly but almost completely in boiling water, affording a solution free from carbonic acid and perfectly saturated with silica.

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* Novum Lumen Chymicum, Amstelodami, 1664. A somewhat earlier mention of the same substance is made by Van Helmont. J. F. Gmelin quotes the following passage from his 'De Lithiasi,' published in 1644:—"Porro lapides, gemmae, arenae, marmora, silices, &c., adjuncto alcali vitrificantur: sin autem plure alcali coquantur, resolvuntur in humido quidem: at resoluta, facili negotio acidorum spirituum, separantur ab alcali, pondere pristini pulveris lapidum."

In making the article for actual use they took for each charge 30 lbs. of well purified potash, 45 lbs. of quartz sand, and 3 lbs. of powdered wood charcoal. This was melted in a refractory crucible, and at the end of five or six hours, when the well fused mass had subsided into a quiet state, they dipped it out with an iron ladle and threw in a fresh charge.

Fuchs found in one sample of the dissolved silicate proportions corresponding to $\dot{K}_2\dddot{Si}_5$. In another analysis he obtained somewhat more silica and less alkali.

They also made a silicate of soda with such relative quantities of materials as should give $\dot{Na}_2\dddot{Si}_3$; but in Fuch's last work, published since the author's death, he recommends 45 lbs. of quartz, 23 lbs. of dry carbonate of soda, and 3 lbs. of charcoal, which would make $\dot{Na}_2\dddot{Si}_5$. Perhaps the recommendation was given on theoretical grounds, for such a product is altogether too hard of solution for practical use. In fact Fuchs, for most of his experiments, took potash waterglass, and the potash silicates are more soluble than the corresponding soda compounds.

Since 1825, owing to the enormous extension of the soda manufacture, the potash and soda salts have exchanged places as far as commercial value is concerned; and now no one thinks of using the potash salts except for those cases in which they are peculiarly suitable. Silicate of soda therefore is the article more commonly employed at the present time, and as found in commerce it varies from $\dot{Na}\dddot{Si}_2$ to $\dot{Na}_3\dddot{Si}_4$.

For designating the substances under consideration the Anglicized German name 'waterglass' is very convenient as a generic term applicable to either the potash or the soda glass, to the solid or the liquid product. It suits the genius of our language and should be generally adopted in preference to the less concise and less accommodating expression 'soluble glass.' It is true, an unpleasant recurrence of sounds may be got rid of by calling a 'solution of soluble glass' 'liquid glass,' but we thus introduce a second term which is no less objectionable because it can be applied to the article only in a single state. The word 'waterglass' is comprehensive enough to express every variety and form of the substance discovered by Fuchs, while it properly excludes silicated alkali or 'liquor silicum,' the "kieselfeuchtigkeit" and "kieselsaft" of the Germans. Yet the two things are confounded by many. Thus while writing this, I have before me the printed directions given by an English manufacturer for dissolving his "soluble glass to produce liquor silicis;" and of two specimens of his glass, one proves to be a sesquisilicate of soda, and the other a bisilicate.

Still though the extremes are unlike enough, there can be only an arbitrary line of distinction drawn on the middle ground. Anything more alkaline than $\dot{\mathrm{N}}\mathrm{a}_2\dddot{\mathrm{S}}\mathrm{i}_3$ is prone to deliquescence, and is too poor in silica for most uses, and it would perhaps be but just to consider 'waterglass' as including nothing more basic than the sesquisilicates. Again with regard to this last term it may be remarked that as silica in combining with the alkalies is regardless of precise atomic ratios, 'silicate' without a limiting prefix, conveys no more definite idea than 'hydrate;' and the world being still divided respecting the atomic constitution of silicic acid, a limiting prefix has to be itself interpreted, unless the context makes its meaning apparent. In this paper silica is taken as SiO_3, merely because custom has made this the more familiar formula.

The fact that silicate of soda is not, like alum or rock salt, an article of definite and invariable composition, is not generally appreciated by consumers, who often absurdly judge of the goodness of a sample by the greater or less time required for dissolving it, and by the hydrometer strength of a solution made from a given weight. A just estimation would take into account the quantity of insoluble matter, the amount of saline contamination, and the relative proportions of soda and combined silica. A dry silicate may have mixed with it no inconsiderable percentage of carbonate, sulphate, sulphid, and chlorid, which greatly impair its quality, though they help to raise the hydrometer. And then again the greater the proportion of alkali, the more quickly may the solution be effected. But for any use to which the article is applied at present, it ought not to contain more than two equivalents of soda or potash to three equivalents of silica, and therefore should properly be rather hard to dissolve.

Manufacture.—It was discovered some years ago that flint or quartz enters into solution when boiled with caustic alkalies under strong pressure; and this method has sometimes been resorted to for manufacturing the silicates of potash and soda.

Thus to obtain a liquid for making 'Ransome's artificial stone,' the direction is given to prepare, in the first place, a perfectly caustic lye with soda ash and lime, and subsequently add enough caustic baryta to decompose the sulphate of soda present. The clear liquor is then to be heated some thirty-six hours to about 300° F., in an iron boiler containing a wire cage filled with broken flints.* The solution is afterwards concentrated by evaporation in open pans. A sample of the substance thus produced is said to have contained 47 per cent of $\dot{N}a_2\ \overset{\cdots}{\bar{S}}i$.† This would be a *liquor silicum*, and altogether too alkaline for most uses.

A very rational improvement of the process is to use infusorial silica or those minerals consisting of silica in its active modification, instead of inert quartz. Bergeat proposes also the fine residue left in the manufacture of sulphate of alumina from porcelain clay.‡

I have not met with any published statement showing the maximum amount of silica which may practically be brought into combination in this way. But even supposing that the alkali could be fully saturated, there are still strong objections to this high pressure system. In fact a more tedious and costly way could hardly be devised, unless we go back to Fuchs' original plan of dissolving precipitated silica in potash lye.

By operating in the dry way, the work may be better done, in less time, and with less expensive materials, less trouble, and less costly apparatus. The easiest method is to fuse sand with carbonate of potash or soda. The cheapest way yet devised,—and the one therefore which seems likely to supersede all others eventually,—is to decompose a sulphate with quartz sand and carbon. It has indeed been suggested that silicate of soda might be made directly from chlorid of sodium, with the aid of overheated steam. It is very doubtful, however, whether any apparatus could be got up, capable of resisting at the same time the very great heat required and the action of the vaporized chlorid of sodium, of the gases passing off, and of the intensely heated silicate remaining. Otherwise such a process might be carried out by passing the mixed vapors of water and salt through an excess of strongly ignited sand,§ provided the product were quite infusible at the temperature of decomposition. But when a substance fluxes, and especially when, like glass, it melts to a tough mass, or at least agglutinates, the action of vapors or gases on it can be only superficial and therefore must be exceedingly slow. In the roasting of ores, for instance, the metallurgist has to exercise the greatest care to keep the matter under treatment in a porous condition, or else many of the metallic sulphids would volatilize sooner than oxydize. And there seems to be no good reason why the same principle should not hold true in oxydizing by steam as in oxydizing by air. The application of Tilghman's process to the manufacture of silicates from simple alkaline chlorids,—however beautiful the method may be in theory,—does not then *a priori* promise advantageous results. The product would at best be too siliceous, and to make it soluble by remelting with carbonate of soda, as proposed by E. Kopp, would add too much to the cost.

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* A figure of the apparatus used is given in Ure's Dictionary of Arts, Manufactures, and Mines,—latest Am. Ed —Art. 'Stone, Artificial.'

† Knapp's Technology.—Am. Ed.,—ii, 398.

‡ Wagner's Jahresberichte über die Fortschritte der Chemisches Technologie,—iv, 205.

§ See this Journal [2], vi, p. 260-266.

Waterglass has sometimes been prepared, as by Fuchs and Pettenkofer, in large crucibles or in the common pots of a glass furnace; and when but little is wanted or a very pure product is desired, this may be the better way. But as the consumption is now very large, and absolute purity is seldom required, most manufacturers use reverberatory furnaces. For fuel I have known anthracite to be burned, though bituminous coal is far better and more economical. Some make an intricate mixture of sand and soda ash by grinding or sifting them together, and heap it up on the bed of the furnace till there is only room enough between the charge and the arch for the products of combustion to pass along,—a little vacant space being reserved just within the charging door. The mixture continually fuses on the surface and runs down to this vacant space, and the fluxed product is drawn out, a little at a time, as it collects. When the whole charge is thus disposed of, a fresh one is thrown in. This method is indeed rude and unsatisfactory, and imposes inconvenient restrictions. Yet under the care of a judicious workman, when the materials are of the best quality and the fire burns well, the result is much better than might be expected. Commonly, however, the silicate so made is exceedingly variable, that of no two drawings being exactly alike. It varies in color from almost white, through every shade of brown, to black. It contains more or less unchanged sand and undecomposed carbonate of soda as well as sulphid and chlorid of sodium and sulphate of soda. In specimens of the article so prepared and intended to be alike, I have found the quantity of dry soda to vary from 24 to 33 per cent, while the uncombined sand sometimes amounted to ten per cent. In this mode of working the consumption of fuel is comparatively small, and the wear of the furnace is slight, but, on the other hand, the thorough mixing of the materials requires no little labor, and the product is rather fritted than completely vitrified.

A better plan is to throw into the furnace but a moderate quantity of the rudely mixed ingredients, keep the whole charge in till it is seen to be well done, and then draw it all at once.

Thus the mass is completely under the control of the workman, perfect decomposition may be insured, and the product can be made uniform in color and quality. In the strong heat, the chlorid of sodium is mostly volatilized, and much of the sulphate is decomposed and changed to silicate. But there usually remains enough sulphid of sodium to communicate a brown tinge. The hotter the furnace is, the easier is it to get a light colored article. The brown color may be completely removed by throwing in a few pounds of arseniate of soda,—or a mixture of arsenic, soda ash, and nitrate of soda,—and stirring it well in just before drawing the charge. Antimoniate or stannate of soda would answer as good a purpose and would be safer to handle. Still I have never known any injury to health to result from using the arseniate.

With a furnace whose bed had an area of 24 square feet, the grate being 3 ft. by 2 ft., and consuming about 83 lbs. of Pictou coal per hour, I have worked four charges in 24 hours, each consisting of 250 lbs. of soda ash,—80 per cent,—and 315 lbs. of pure quartzose sand. The well fused mass was decolorized with about 4 lbs. of arseniate of soda, and then drawn out into a kettle full of cold water kept constantly renewed. As foreign salts can only be mechanically mingled with the melted glass, any particles of sulphate of soda that have escaped decomposition, are dissolved and washed away as the fluid mass breaks up into minute fragments by contact with water. The glass so obtained had a very slight greenish color and was quite pure.

To make a more readily soluble sesquisilicate for calico printers, the charges consisted of 260 lbs. of soda ash and 250 lbs. of sand.

Fuchs directs an addition of coal dust to be made to the materials, but when the heat is strong, such an addition is entirely unnecessary. The drawing into cold water causes but little loss when we have bisilicate of soda to deal with, though with anything more alkaline than sesquisilicate of soda or bisilicate of potash, the waste is too considerable. When waterglass is to be dissolved at the manufactory, it may as well be drawn into water, for this saves the expense of grinding. But when it is to be sold in the solid form, water drawing cannot be recommended; since after the silicate has once been wet, it is almost impossible to get it dry again, and if it is packed in casks, the particles soon cohere into one solid, unmanageable lump. To avoid such a difficulty the melted mass may be received into a thick cast iron pan or half cylinder, and, when cold, broken up and ground in suitable mills. The best apparatus for grinding would be a large edgestone mill. A series of toothed crushing rollers made of chilled cast iron answers very well. Common Buhr mill stones wear away too fast.

When drawing into water is deemed advisable, the workman should always make sure that the finished charge in the furnace contains no foreign saline matter visible as a limpid liquid amidst the viscous glass; for this thin liquor will produce violent explosions as it touches the water, though the silicate itself behaves very quietly.

The use of Glauber's salt instead of carbonate of soda in making window glass,—a substitution which was successfully carried out by Baader in 1808,—naturally suggests the employment of the alkaline sulphates for preparing waterglass. By some experiments made thirteen years ago with reference to this matter, I found that two equivalents of simple sulphate of soda required not less than three equivalents of sand for the complete expulsion of the sulphuric acid; and further trials made in the large way have gone to confirm this result. The decomposition of sulphate of soda is much less difficult when lime, alumina, or another sulphate is present. Thus one equivalent of sulphate of baryta, one eq. of sulphate of soda, two eqs. of carbon, and two eqs. of silica, melted easily to a perfect glass.—One eq. of sulphate of baryta, one eq. of sulphate of soda, two eqs. of coal, and three eqs. of sand, required a stronger heat.—One eq. of sulphate of baryta, two eqs. of sulphate of soda, three eqs. of carbon, and three eqs. of silica, fused easily to a clear glass.—One eq. of carbonate of lime, one eq. of sulphate of soda, one eq. of coal, and two eq. of silica were also vitrified with little difficulty. Feldspar,—$\overset{\dots}{\bar{\mathrm{A}}}\mathrm{l}\,\dot{\mathrm{K}}\,\overset{\dots}{\bar{\mathrm{S}}}\mathrm{i}_4$,—with the aid of carbon, decomposes nearly three eqs. of sulphate of soda, giving a very 'short' product which when powdered is readily attacked by acids, and might be used in the manufacture of alum.

The vitrification of sulphate of soda or potash, requires more heat, more time, and more skill than the production of waterglass from the carbonates; yet the far greater cheapness of the sulphates renders their use more economical. It is better to take purified sulphate of soda made by dissolving the crude article, precipitating the iron with lime, and evaporating the clear solution to dryness. This however involves time, labor, space, and extensive apparatus; while a silicate pure enough for ordinary use may be prepared directly from the clean residue of the nitric and muriatic acid manufacture. The iron, lime, and magnesia contained in the crude sulphate, do indeed somewhat impair the solubility of the product, yet when it is a sesquisilicate, it can be dissolved by hard boiling. If a little soda ash is added to the charge in the furnace, after the decomposition of the sulphate is finished, the subsequent solution is considerably facilitated.

The heat required is nearly or quite as great as that of an iron puddling furnace; and the melted materials act severely on the interior of the furnace,—especially on the side walls just at the surface of the fused charge. A large allowance must therefore be made for 'wear and tear.' The furnace should be substantially built, the interior being constructed of very tough and compact fire bricks rich in alumina; and it would no doubt be advantageous to line the flue and the sides of the bed with some hard pressed and well burned blocks of the same material as glass pots. As economy of time and fuel requires a rapid consumption of coal, the grate surface must be large, say equal to one-fourth of the area of the bed, and the throat over the bridge should not be too contracted. To avoid the vexation of a leaking fire bridge, it is best to have the working bridge about four inches lower than the level of the grate. The sills of the charging and discharging doors may be on the same plane as the grate, and a gentle slope should connect the sill of the discharging door with the general level of the bed.* Were the doors not thus raised, the melted sulphate of the intumescent glass would flow out unbidden. I have used a furnace with a working sole 40 square feet in area, and a grate surface of 11 square feet. The chimney is about 50 feet high, and the arched flue leading straight into it, is 9 inches high in the centre, and 20 inches wide. In such a furnace four charges are worked in 24 hours, with a consumption of about 5000 lbs. of Pictou coal, —each charge consisting of 550 lbs. of white sand, 600 to 700 lbs. of crude sulphate of soda, and 70 to 90 lbs. of anthracite coal dust.† The approximate amount of carbon is best determined by actual trial. But on account of varying accidental influences no two charges are found to require precisely the same weight of coal. It is therefore best to reserve a few pounds to be thrown in afterwards, or not, according to circumstances. When the charge is well fused and the sand has all disappeared, if there is an excess of coal, the glass will remain black. If there is a deficiency of carbon, the mass will gradually become light colored,—as is shown by samples taken out from time to time on the end of an iron rod,—and on stirring, the sulphate will be seen to separate, as a thin liquid, from the pasty silicate. The workman should then throw in a pound or two of coal and stir well. The mass is now suddenly puffed up with the escaping sulphurous acid, but soon subsides again. A second or third addition of coal may be necessary, and some practice and tact are required on the part of the workman that he may stop at just the right point. An excess of sulphate can be remedied, but an excess of coal is hard to get rid of. When the melted mass has become smooth and homogeneous and light colored, it may be decolorized with arseniate of soda and drawn out, the hoes being changed as they get hot and pliant.

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* The plan of having the bed lower than the grate may be highly recommended when there is a material to deal with, that becomes fluid in a strong heat. Thus I have found it to be very advantageous in smelting artificial sulphate of lead with iron borings and coal. A low sole necessitates a high arch, but when the arch slopes regularly downward from the highest point over the bridge to a low and wide flue, the consumption of fuel is found to be no greater than in a furnace of the more common form.

† When soda ash is used instead of the sulphate the same furnace affords six somewhat larger charges in 24 hours.

For making a liquid or pasty silicate, the glass coarsely ground may be boiled with water till the solution stands at about 25° B. after cooling. If it is made much stronger it will not settle readily. Some dissolve waterglass by blowing steam directly into the water, but in that case the solution goes on very slowly, because the heat is insufficient. It is far better to have an iron kettle heated by a fire. After being allowed to deposit the insoluble matter, the liquid may be concentrated by evaporation to any required degree within certain limits. When it becomes thick, the further application of heat is attended with no little difficulty; for the silicate then rapidly adheres to the kettle, and there is needed very diligent scraping with a chisel-pointed bar to keep the bottom and sides of the vessel clear of the spongy coating. And to push the evaporation to dryness, is quite out of the question. Sesquisilicate of soda cannot conveniently be made stronger than 50° B.

When we wish to obtain the greatest possible relative amount of silica in solution, it is necessary to make the glass of none but the purest materials. Earthy or metallic oxyds very much lessen the solubility of the product, and if more than a mere trace of them is present, a larger proportion of alkali is required to render the mass capable of yielding readily to boiling water. A silicate so contaminated will not in fact enter directly into solution; it is only decomposed by boiling with water, into a more alkaline silicate which dissolves and a compound earthy silicate which remains as a bulky residue, often in the form of plates or scales. Thus of a well worked waterglass made from crude sulphate of soda, water took up only 89 per cent, leaving a copious sediment consisting of soda, lime, magnesia, alumina, ferrous and ferric oxyds, and silica. Indeed common glass is but an alkaline silicate rendered insoluble by a more considerable amount of lime or oxyd of lead. Fuchs himself pointed out the necessity of having the sand free from lime and alumina. He states that a little iron does no harm; but this can be true only when the glass is left brown or black so that the iron exists in the state of sulphid. Fuchs says that with pure quartz and pure potash an insoluble glass cannot be prepared. "For if we take two parts of quartz to one of potash, we obtain,—as I have convinced myself,—a glass which partly dissolves in water." This would make about $\dot{K}_3\ddot{Si}_{10}$. "Besides even glass containing lime is more or less attacked by boiling water, as has been long known, and as Scheele especially proved. I found that many a glass, when it is rubbed a long time with water in an agate mortar, reacts very sensibly alkaline, and that when finely powdered glass is boiled for many hours with water, a fluid is obtained which has an alkaline reaction and gives a flocculent precipitate with sal ammoniac." Pelouze found that when good white glass,—$\dot{N}a_8\,\dot{C}a_{11}\,\dddot{S}i_{32}$,—was finely pulverized and boiled for some time with water, about three per cent was dissolved. Another kind, containing a less proportion of lime, ($\dot{N}a_4\,\dot{C}a_2\,\dddot{S}i_{13}$,) yielded to boiling water 18·2 per cent, and what entered into solution proved to be sesquisilicate of soda.* And Fresenius affirms that even a glass vessel yields a ponderable amount of its substance to water which is boiled in it only a short time. Still a simple potash or soda silicate with three equivalents or more of acid to one of base, may be considered as practically insoluble. Thus Peligot found the so-called 'alabaster glass' to consist almost entirely of silica and potash in such proportions as to make $\dot{K}\,\dddot{S}i_5$, were the silica all in combination. But in this case a part of the silica is merely diffused, and so produces the opacity of the glass. Stein however found besides, 1·5 per cent of lime and 2·3 per cent of bone earth, in alabaster glass.†

Analysis.—When a silicate is in lumps or coarse powder, we can judge as to the absence of foreign salts by the degree of transparency. A well made article is clear, bright and homogeneous. One not properly prepared has a dull, resinous appearance; and if there is much sulphid or chlorid present, the mass will be spotted, streaked, cloudy, or entirely opaque. Less than ten per cent of intermixed saline matter, is sufficient to render waterglass quite milky.

But the real value of any sample can be ascertained only by a chemical examination, and as the analysis is liable to some peculiar chances of error, it may be well to point out a few of the peculiar precautions to be observed.

In the case of a dry glass, especial pains must be taken to insure the solution of everything that water will take up,—a matter requiring some labor and time. For determining the amount of alkali, I take an average of the specimen to be tried and patiently rub some, in a very hard, four inch Wedgewood mortar, to an exceedingly fine powder,—grinding only about a gram at a time, and being careful to keep it as dry as possible. Five grams or more are then weighed out and boiled with forty or fifty times as much water, in a porcelain dish, for a time, varying from fifteen to ninety minutes, as occasion may require. To prevent a violent bumping, the mixture must be stirred briskly and without intermission, during the whole time of heating. If there is no uncombined sand in the article under treatment, the cessation of the gritty feeling, shows plainly when a farther application of heat is unnecessary. For a well fluxed sesquisilicate of soda, 15 minutes boiling is quite sufficient. Any uncombined sand present in a sample, causes an uncertainty in the time required. Should there be a gritty residue after half an hour's boiling, it is advisable to stop and make a preliminary testing. If we find less than 28 per cent of alkali indicated, it is best to start anew and heat at least an hour and a half.

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* Liebig and Kopp's Jahresbericht for 1856, p. 354.

† Wagner's Jahresbericht,—iv, p. 244.

After the solution is completed, the whole should be made up to a given weight,—being at least forty times that of the dry silicate,—covered, and allowed to get perfectly cold. The larger part of the liquor being then poured off, without disturbing the sediment, a suitable proportion of the whole amount may be weighed out and tested by any of the common alkalimetric methods. Filtration is tedious and unnecessary, for though the decanted liquid is seldom perfectly clear, even after long repose, the quantity of suspended earthy matter is altogether too insignificant to influence the correctness of the result. Waterglass which is already in the liquid or pasty state, must be largely diluted before trial, since a strong or a warm solution is apt to gelatinize before all the acid is added, and then exactness is impossible, because the thick jelly is not readily penetrated by the acid subsequently dropped in. Therefore should gelatinization occur, a new trial must be made with a weaker or colder solution.

When the operation is finished, if sulphuric acid has been used, the tested liquid itself can be dried down, and then the silica is left in a coarse granular form, easy to wash, collect, ignite and weigh. If the precipitation of the silica is effected with an ammonia salt, the residue is likely to be very bulky, and so fine and light that it requires no little care to keep it from blowing partly away during ignition.

An excess of nitric or chlorhydric acid added to very dilute waterglass, causes no apparent change for a long time, and in a weak solution so treated we may test for sulphates or chlorids, for iron or for arsenic acid, without previously removing the silica. For the quantitative determination of sulphate and chlorid it is best to treat the silicate solution with an excess of nitrate of ammonia instead of an acid, and thus prevent any loss of chlorine in drying down.

If sulphids are present in a liquid waterglass, a small bit of sulphate or carbonate of lead dropped in will soon become discolored.

When the per-centage of water is to be ascertained, it is not well to heat the solution by itself, for in such a case it finally expands to an enormously bulky, vesicular, unmanageable mass.

A known quantity of freshly ignited sulphate of lime should first be stirred in, the weight of the sulphate being at least twice as great as that of the dry alkali supposed to be present. Then in a few moments a double decomposition is effected and the glutinous nature of the silicate is destroyed. The whole becomes stiff and crumbly, and the water may be driven off with great ease, while everything except the water is retained.

As all waterglass contains in combination more or less of the earthy silicates which remain as a light sediment after dissolving, the amount of unchanged sand can be found only by resorting to a mechanical separation. For this purpose a quantity of the coarsely ground silicate should be boiled with water till everything soluble is taken up, and then the liquor being stirred and allowed to settle for a moment, may be decanted, carrying with it the suspended silicates. The gritty residue left after one or two washings conducted in the same way, represents pretty nearly the amount of uncombined silex.

[To be continued.]

Art. XVII.—*Sketch of the Distribution of Forest-trees in Nebraska Territory;* by James T. Allan. (In a letter to Prof. Gray.)

The relative proportion of the several species and their general distribution in the tract of country lying west of the Missouri and between the paralells 40° and 43° is all that I will attempt to describe in this sketch.

And one remark will apply to all this country, that timber is found only upon streams or small groves about some spring on the wide prairie.

The valley of the Missouri is from three to six miles wide, and sometimes the stream winds down near the centre with both sides fringed with willows, behind which is a belt of cottonwood (*Populus monilifera*); these trees often 80 to 100 feet high: upon the bluffs which wall each side of this valley we find the different varieties of hard wood, also upon the hills, and ravines opening toward the river. On the tops of these hills we find *Quercus alba* and *rubra*, with occasional trees of *Quercus coccinea;* half way down the steep sides of these ravines we find *Tilia Americana* and *Ulmus fulva* in about equal quantities, with clumps of *Carpinus Americana*. Still lower down and in the rich soil at the bottom are *Gymnocladus Canadensis*, *Celtis occidentalis* and *Fraxinus Americana*, while on the cool northern slope half hanging down the hill are plenty of *Staphylea trifolia* and *Rubus occidentalis*. As we recede from the river toward the summit of the ridge we find scattering trees of *Carya alba* among the Oaks before mentioned, till we come to the prairie where the trees terminate with a few "Scrub Oaks," 12 to 20 feet high, standing beyond the fringe of *Corylus Americana.* Among the latter in the spring we discover the bright blossoms of the Red bud (*Cercis Canadensis*) and in autumn the bright seed-pods of the *Euonymus.*

Going west from the Missouri we find no trees except on the small streams on which, at intervals of ten miles or more are groves of White oak, Bur oak, *Carya glabra* and sometimes White elm; these groves will average in extent from 100 to 500 acres, and the above mentioned, with *Juglans nigra*, make up their prominent trees.

The valley of the Platte, to which so much attention is now directed as the great central route, demands a notice. At the mouth we find a heavy body of timber, chiefly Cottonwood, with a small proportion of *Acer rubrum* and *Morus rubra.* As we proceed up 20 miles the dull green of *Juniperus Virginiana* begins to be seen, which farther up we see covering some of the small islands, with trees often 18 inches and more in diameter. This has furnished for a hundred miles or more telegraph poles of a superior quality.

Upon the tributaries of the Platte, particularly on the north side *Quercus macrocarpa* is the most abundant of large trees.

After leaving Fort Kearney, the immigrant finds but a fringe of cottonwoods skirting the stream, and on the road to the new gold mines for 200 miles not a tree for shade or wood: the well known "Buffalo chips" must supply the latter. Upon the North Platte the emigrant to California or to the Mormon "Zion" is always in sight of the trees bordering on the stream, though often too far off to obtain fuel. 400 miles beyond Ft. Kearney is found in several places large groves of *Negundo aceroides* on the banks of the creeks.

After passing Fort Laramie the pleasant sight of green pines, though at a distance, relieves the eye and tells of the cool waters in the south pass, so refreshing in the heat of summer.

The very rapid growth of trees in this rich soil is a noticeable feature. The hazel which fringes the timber on the prairie side is interspersed with abundance of saplings *Carya alba* and *glabra* and *Alnus fulva*, which shoot up with wonderful rapidity, while upon the sand bars of the river, as soon as the waters subside in July, there spring myriads of young cottonwoods and willows. Of the latter I have neglected to speak though they cover no inconsiderable portion of the valleys of the Missouri and Platte. Everywhere fringing the streams, and where there is a tract annually overflowed by the spring rise there we find a dense growth often 20 to 30 feet high and from one to three inches in diameter growing so thickly that it is impossible without great difficulty to force a passage through them.

The relative proportion of the several species may perhaps be set down as follows:

1. *Populus monilifera.*
2. *Quercus macrocarpa.*
3. *Quercus alba* and *Quercus rubra.*
4. *Tilia Americana, Ulmus fulva* and *Quercus discolor.*
5. *Juglans nigra, Ulmus Americana.*
6. *Carya alba, Carya glabra.*
7. *Fraxinus Americana, Celtis occidentalis.*
8. *Juniperus Virginiana, Platanus occidentalis.*
9. *Acer rubrum, Gymnocladus Canadensis.*

Besides the above mentioned, of the smaller varieties there are *Prunus Americana, Zanthoxylum Americanum, Staphylea trifolia, Negundo aceroides, Corylus Americana, Carpinus Americana, Alnus incana, Euonymus Americana, Cercis Canadensis, Cornus sericea ?*

In the Omaha land district, which contains something like 4000 square miles of land, there appears from the plots in the office to be about 75,000 acres of timber. A tract of country of equal size lying west of it would contain much less.

The botanical names above corrrespond to and have been compared with the descriptions in Gray's "Manual."

Omaha, Nebraska, April 2, 1861.

Art. XVIII.—*Remarks on the Age of the Goniatite Limestone at Rockford Indiana, and its relations to the "Black Slate" of the Western States, and to some of the succeeding rocks above the latter;* by F. B. Meek and A. H. Worthen, of the Illinois State Geological Survey.

It is known to most of those who have studied the geology of the west, that there is an outcrop of limestone near Rockford, Indiana, usually termed the Rockford Goniatite bed, in regard to the age of which there is some difference of opinion amongst geologists. This rock is only exposed at a single locality in the bed of a small stream, where not more than a thickness of two feet of it is seen. It is a mottled brownish and ash-colored argillaceous limestone, and contains, in addition to the *Goniatites* from which it takes its name, other fossils belonging to the genera *Nautilus, Orthoceras, Pleurotomaria, Euomphalus, Spirifera, Rhynchonella,* &c.

No other rock is seen above or below this at the Rockford locality, but in sinking wells in that vicinity it has been ascertained that it rests upon a Black Slate forming a marked horizon in several of the Western States. Owing to the fact that this slate has been seen to form the bed the same stream at another place about one mile above, and at a slightly higher elevation, it has been supposed that the Goniatite bed is intercalated in it, and the mystery has been that the fossils occurring in the limestone have a decidedly Carboniferous aspect, and in some instances have even been considered actually identical with well known European Carboniferous species; while the Black slate has been referred by Prof. Hall of Albany, New York, to the horizon of the Marcellus shale of the N. Y. series, occupying a position far down in the Devonian, at the base of the Hamilton Group.* In addition to this, Prof. Hall also refers a rather extensive series of fine arenaceous, and more or less argillaceous deposits, holding a position above the Black slate near Louisville, Ky., and at other localities in the west, to the Portage and Chemung Groups of New York, higher members of the Devonian, which are in New York overlaid by a great thickness of upper Devonian strata equivalent to the Old Red Sandstone, of the British geologists.

Without attempting to give a detailed statement of the opinions that have from time to time been expressed in regard to the age of the Black slate and overlying strata alluded to, we would remark that the most generally received opinion amongst western geologists has been, that the whole series known in Indiana and Kentucky as the "Fine-grained sandstone of the Knobs," down to the Black slate, should be included in the Carboniferous system, and some even include the latter also in the Carboniferous.

When the distinguished French geologist, DeVerneuil, was in this country in 1846, he made an excursion through the Western States for the purpose of studying our rocks, and obtained a fine collection of their characteristic fossils. After his return to Europe he published a highly interesting memoir on the parallelism of American and European formations, in which he referred all the fine arenaceous and shaly beds holding a position between the Black slate and the Carboniferous limestones in Indiana, Kentucky, and Tennessee, as well as a portion of the Waverly sandstone in Ohio, to the Carboniferous system. One of the *Goniatites* given to him by Dr. Owen from the Rockford limestone, he considered identical with *G. rotatorius* of Koninck, a well known Lower Carboniferous species, which led him to refer the limestone from which it was obtained to the lower Carboniferous. The Black Slate, however, he referred to the horizon of the Genesee slate of the New York series.†

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* See Proceed. Am. As. Geol. and Nat., p. 267, vol. i; also Rept. 4 Dist. N. Y. Geol. Survey, p. 519.

† See Bulletin Geol. Soc. France, vol. iv, 2d Series.

After the publication of this paper, Prof. Hall was for a time inclined to concur, at least in part, with De Verneuil's views, but he subsequently returned to his former opinion, which he has continued to maintain, with his usual zeal and ability, to the present time. In an important paper recently published by him in the 13th Annual Report of the Regents of the University of New York, he describes a number of fossils from the Rockford Goniatite bed, along with others from the Marcellus shale in New York, and refers not only the Black slate, but the Rockford limestone, which has been supposed to be intercalated in it, to the horizon of the Marcellus Shale.

As the question in regard to the age of the Rockford goniatite bed has an important bearing on the parallelism of our rocks with established horizons elsewhere, we have, while investigating the fossils in the Illinois State Geological collection, carefully compared them with a series of specimens from Rockford, Indiana, in order to determine whether or not the Goniatite bed at that place is represented in our state, and if so, what relations it bears to our other rocks. These comparisons have led us to the conclusion that it is represented in Illinois, as well as in Missouri and Iowa, and that it holds a much higher stratigraphical position than that assigned it by Prof. Hall.

In the first place, we should remark that there is no evidence whatever, that the rock under consideration is overlaid at Rockford, or at any other locality, by any part of the Black slate, though we know the slate occurs beneath it there, as well as in Illinois.

Again after a careful study of a series of the Rockford fossils, we have clearly satisfied ourselves that the limestone from which they were obtained is of the same age as the Chouteau limestone of Prof. Swallow. The evidence of this is the positive identity of at least six of the 23 or 24 known Rockford species, with characteristic forms of the Chouteau limestone in Missouri and Illinois, while most of the others are either identical or closely allied. The following is a list of the species from Rockford which are known to be identical with Chouteau limestone species found in Missouri and Illinois, viz:—*Nautilus digonus*, Meek and Worthen, *Euomphalus lens*, Hall, *Rhynchonella Missouriensis*, Shumard, *Spirifera Cooperensis*, Swallow, (=*S. semiplicata*, Hall) *Cardiopsis radiata*,* Meek and Worthen, (=*Megambonia Lyoni*, Hall,) and *Sphenopterium enorme*, Meek and Worthen.

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* We placed this species at first, provisionally, in the genus *Cardiomorpha* of Koninck, but suggested at the same time that we suspected it to be generically distinct. Later examinations have satisfied us of the correctness of this suggestion, and we have proposed to establish for its reception a new genus, under the name of *Cardiopsis*, from its resemblance to *Cardium*. It has no near relations to the type of the genus *Megambonia*.

In classifying the rocks of Missouri, Prof. Swallow, (and his assistants, including one of the writers) knowing that his Chouteau limestone holds the same position, and contains many of the same fossils found in beds elsewhere in the west referred by Prof. Hall to the Chemung group, placed it on a parallel with the Chemung, and Prof. Hall has also since referred equivalent beds in Iowa to the same horizon.

In order that the reader may understand more clearly the position of the Chouteau limestone with relation to our other western formations, we give below a section showing the order of succession of the several beds, beginning with the Burlington limestone, which is acknowledged by all to be Carboniferous, and extending down to the Hamilton Group inclusive:—

1.—Burlington limestone attaining a thickness of	200 feet.
2.—Chouteau limestone,	100 "
3.—Vermicular sandstone and shale,	65 to 100 "
4.—Lithographic limestone (rather local),	60 "
5.—Black Slate,	30 to 40 ft.[*]
6.—Hamilton group,	120 "

Numbers 2, 3, and 4 of this section were included by Prof. Swallow in the Chemung, though Prof. Hall thinks that at least a part of the Lithographic limestone, should be referred to the Hamilton group. It is true a few of the many fossils found in the Lithographic limestone resemble Hamilton forms, and one of them seems to be undistinguishable from *Orthis Vanuxemi*, of Hall, a common Hamilton group species. It is well known, however, that many of the other fossils found in this limestone occur in the Chouteau beds above, where they are sometimes mingled with Carboniferous forms. At the same time, unless the *Orthis* just mentioned is an exception, not one of the species occurring in the Lithographic limestone and the Chouteau beds above, can be, so far as our knowledge extends, positively identified with any of the numerous well marked Hamilton forms in the beds below the Black slate.

In regard to the occurrence of the *Orthis* which seems to be undistinguishable from *O. Vanuxemi*, in the Lithographic limestone, we would remark that we do not think it a fact entitled to much weight, when it is borne in mind that *O. Vanuxemi* is so very closely allied to the Carboniferous *O. Michilini* of L'Eveille, that even Mr. Verneuil regarded them as scarcely distinguishable, and the differences between them have not yet been clearly defined. Prof. Hall acknowledges that it "is so closely allied to *O. Michilini* of L'Eveille, that it is very difficult to point out characters by which it may be distinguished." He thought, however, that, judging from DeKoninck's figures of *O. Michilini*, he could see some slight differences in the vascular impressions and dental processes, and remarked that the "minute granulations or punctæ upon the exterior surface [of *O. Vanuxemi*] present characters which are not noticed in the figures and descriptions [of *O. Michilini*] so far as I have observed."* Yet we find in Mr. Davidson's very carefully written description of *O. Michilini*, since published in his Monograph of the Carboniferous Brachiopoda of Scotland, that he says the surface is "covered with minute punctures." In fact almost every word of Mr. Davidson's description would apply equally well to *O. Vanuxemi;* and after a very careful comparison of our western form with authentic specimens of *O. Michilini* from Scotland, sent by Mr. Davidson, we have been unable to find any appreciable differences. We are not, however, contending that they really are identical, but we simply mean to say that it is unsafe to base conclusions upon such closely allied species.

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[*] In some parts of Ky. and Indiana, the Black slate attains a thickness of about 100 feet.

The three subdivisions Prof. Swallow has called Vermicular sandstone and Shale, and the Chouteau limestone above, although often distinctly separable by their lithological characters, seem sometimes to replace each other, or often one is augmented in thickness at the expense of the other, though they are mainly characterized by the same fossils. The whole group is very variable in its lithological characters, the very same beds that are at one place composed of hard bluish or ash-colored compact limestone, being at others a yellowish very fine soft argillaceous, or arenaceous rock, or consisting of alternations of brownish and greenish shaly limestone, &c. The fossils are usually in a better state of preservation in the limestones than in the other beds, and assume a more decided Carboniferous aspect than in the arenaceous and argillaceous strata, even where the latter occur above the limestones.

The Black slate, so far as our knowledge extends, seems to be nowhere greatly developed in Missouri, though it is probably represented at Hannibal in that state by six feet of blue shale beneath the Lithographic limestone, as may be seen by Prof. Swallow's Section No. 15, p. 99, of the Missouri Report. Some eight or ten miles east of Hannibal, in Illinois, it is seen occupying the same position with relation to the Lithographic limestone, and attaining a thickness of thirty or forty feet.

Although the position of the well marked Hamilton beds with relation to the representative of the Black slate, have probably not been very clearly determined in Missouri, they are there known, as Prof. Swallow has shown, to hold a position beneath the Lithographic limestone. In Illinois, however, numerous exposures show that the black slate comes in just above all the well defined Hamilton Group beds as may be seen by the following section taken near Jonesborough, Union county, Illinois:—

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* Tenth Ann. Rept. Regents University of N. Y., p. 136.

1. Brown silicious shale probably representing the Vermicular sandstone and Shale of Prof. Swallow,	50 to 60 ft.
2. BLACK SLATE with its characteristic *Lingula*,	40 to 50 ft.
3. HAMILTON GROUP, consisting of a dark limestone, containing *Phacops bufo? Strophomena Hamiltonensis*, *Trepidoleptus carinatus*, *Heliophyllum Halli*, and other well known Hamilton species, along with *Atrypa reticularis*, &c.,	120 ft.

Now as the Chouteau limestone of Prof. Swallow, is known to hold a position immediately beneath the Burlington limestone, and considerably above the horizon of all the Hamilton group beds of the West, as well as above the Black slate, which also overlies the Hamilton group in Illinois, it follows as a matter of course, that its representative in Indiana, the Goniatite bed at Rockford, cannot belong to any part of the Marcellus shale, lying at the base of the Hamilton group.

Again, the fact that all our well marked Hamilton beds in Illinois, hold a position beneath the Black slate, while in New York, as already stated, the Marcellus shale lies at the base of the Hamilton Group, renders it very improbable that even our western Black slate formation represents the Marcellus shale. Its position would seem to be more nearly that of the Genesee slate of New York, as suggested by M. DeVerneuil, which supposition is strengthened by the affinities of the only fossils yet found in it, viz,—a small *Lingula* and a *Discina*, scarcely, if at all, distinguishable from species occurring in the Genesee slate of New York, to which in fact they were referred by Prof. Yandell and Dr. Shumard in 1847.

Although Prof. Hall describes about twenty species of fossils from the Rockford *Goniatite* bed, it will be observed that he does not identify any of them, with Marcellus shale species, or with forms occurring in any other part of the Hamilton group either in New York or in any of the Western States; but refers the rock in which they occur to the horizon of the Marcellus shale, solely from the fact that it was thought to be intercalated in the Black Slate, and from the supposed affinities of its *Goniatites*, to Marcellus species. Aside, however, from the evidence we now have that the Rockford limestone is of the same age as the Chouteau limestone of Missouri, which comes in some distance above the Black slate, and is exactly equivalent to beds referred by Prof. Hall at Burlington, Iowa, to the Chemung, its whole group of fossils including the *Goniatites*, strike us as being more nearly allied to Carboniferous forms than to those of any of the New York rocks.

If we take for instance the three species of *Goniatites* now known from the Rockford bed, we find first that *G. Lyoni*, Meek and Worthen, (=*G. hyas* of Hall,) which Prof. Hall compares with *G. Chemungensis*, of Vanuxem, differs materially from Vanuxem's figure of that species, in having but two lobes in each septum visible on either side, while *G. Chemungensis* is figured as having five. It bears no near analogy whatever, to any of the species figured by Prof. Hall, from the supposed equivalent bed in the Marcellus shale of New York.

Again *Goniatites Oweni* of Hall,* is much more closely allied to *G. princeps* of Koninck, (a European Carboniferous species) than it is to any of those yet known from the Marcellus shale, or any other rock in New York. It agrees in size and form with Koninck's species, as well as in having constrictions at intervals left upon its internal cast by the occasional thickening of the lip, while it presents but slight differences in the lobes of its septa; the most obvious difference being in the length of its dorsal lobe, which is more attenuated than in *G. princeps*, though even in this character, the Rockford specimens vary to some extent. It is true *G. princeps*, as figured by Mr. Koninck, is marked by regular costæ, while *G. Oweni*, as we usually see it in the condition of worn casts, seems to be smooth. We have, however, seen unmistakable traces of similar costæ, on some of the Rockford specimens. We are not contending, however, for the absolute specific identity of these forms, but we do maintain that the analogy of the Rockford species to *G. princeps* is much nearer and more striking than its relations to any of the Marcellus shale species.

The third species, *Goniatites ixion* of Hall, while it presents but very remote analogy to any of the Marcellus shale species, is scarcely, if at all distinguishable from the Carboniferous species, *G. rotatorius*, of Koninck,† with which Mr. Verneuil considered it identical. They may possibly belong to different species, yet the minute differences pointed out by Prof. Hall, are, we think, not inconsistent with specific identity in a genus like this. Every one who has studied the *Ammonitidæ* must be aware how unsafe it is to base specific distinctions on minute differences in the smaller details of the lobes of the septa, or upon slight differences in the size of the umbilicus. We are not insisting, however, upon the exact identity of these forms, for that is a question that can only be settled by direct comparison with authentic specimens of the European species; yet that they are at least strikingly analogous, must be evident to any one who will but glance at Prof. Hall's, and Prof. DeKoninck's figures.

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* See thirteenth Ann. Report Regents University New York, p. 100.

† Animaux Foss., pl. 51, fig. 1.

If we compare the two species of *Nautilus* now known, from the Rockford beds, *N. digonus*, and *N. trisulcatus*, Meek and Worthen, we find them wholly unlike any forms known in the Marcellus shale, while the latter species is allied to the Carboniferous *N. sulcatus* of Sowerby, (see Koninck's An. Foss., pl. xlvii, fig. 10), and the former is analagous to two or three of the species figured by DeKoninck from the Carboniferous rocks of Belgium. In fact both of these species belong to a peculiar subgenus of *Nautili* for which we have proposed the name *Trematodiscus*, a group embracing a number of discoid Carboniferous species, with a wide perforated umbilicus and narrow whorls, which are ornamented with longitudinal angular ridges, and sometimes with parallel striæ. So far as we know, the entire group is confined to the Carboniferous system, unless those found in the Rockford *Goniatite* bed and its equivalent in Illinois, are exceptions.*

In order to show the close relations between the Chouteau limestone and the Burlington (acknowledged Carboniferous) beds above, in Illinois, we might give many sections, with lists of fossils, but the following taken near Grafton in Jersey county, will be sufficient for illustration:—

1.—BURLINGTON LIMESTONE,

presenting its usual characters and containing its usual fossils; showing at the immediate outcrop where the section was taken, a thickness of 8 to 10 feet, but increasing as the country rises back to a thickness of 150 or 200 feet.

2. CHOUTEAU LIMESTONE.

Upper part consisting of ash-colored argillaceous shelly limestone in thin lares, with greenish marly partings and calc spar concretions. Containing *Cyathoxina* (undt. sp.), *Poteriocrinus Meekinus*, *Actinocrinus* (undt. sp.), *Spirifera peculiaris*, *S. grimesi*, *Athyris Prouti*, *A. Hannibalensis*, *Strophomena rhomboidalis*, *Orthis Michilini*, *O. resupinata*, *Productus semireticulatus ? Platyceras equilatera*, - . - - - - - - 40 feet.

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* The forms included in this subgenus differ so widely from the recent typical species of *Nautilus*, that few conchologists would place them in the same genus if they were found living in our present seas. It includes such species as *N. pingius*, *N. stygialis*, *N. Edwardsianus*, and *N. Omalianus* of Koninck, and *N. sulcatus*, and *N. cariniferous* of Sowerby. *Gyrocerus gracile* of Hall, from the Rockford limestone probably also belongs to this group.

Lower beds, composed of brownish-buff colored argillaceous limestone, with nodules of calc spar, the upper part being in thicker beds than the lower which has a more greenish tinge. Fossils—*Strophomina rhomboidalis, Chonetes ornata, Cardiopsis radiata, Productus semireticulatus? Cyrtoceras* and *Gomphoceras* (undt. sp.) *Nautilus digonus*, a discoid *Goniatite*, and a *Proetus* (undt.) - - 60 feet.

3.—Black slate shading upwards to greenish shale, - - 40 "

4.—Hamilton group, consisting of dark limestone, - - 10 "

5.—Niagara group limestone of buff col., - - - - 100 "

Of the fossils mentioned in the above section the following are usually characteristic of the Chouteau series:—*Cyathoxina* (undt.), *Poteriocrinus Meekianus.** *Spirifera peculiaris, Athyris Prouti, A. Hannibalensis, Chonetes ornata, Cardiopsis radiatus, Nautilus digonus*, and an undetermined *Goniatite.* Those usually regarded as strictly Carboniferous are,—*Spirifera Grimesi, Orthis Michilini, Productus semireticulatus*, and *Platycerus equilatera.*

The forms common to Carboniferous and Devonian rocks, are *Strophomena rhomboidalis*, and *Orthis resupinata.* The species here enumerated, however, are only those collected at the locality where this section was taken. Several other Carboniferous forms are sometimes found in the Chouteau beds at other places, and a number of other Chouteau species are known to range up into the Burlington beds. From a locality in an adjoining county, we have an *Actinocrinus*, from the Chouteau series, which we can only distinguish as a variety, from the well known Burlington *A. pyriformis* of Shumard; and along with it we also find a species of a peculiar coral (*Sphenopoterium enorme*) described by us from the Rockford Goniatite bed.

In an interesting and important paper on the rocks and fossils of Burlington, Iowa, published by Mr. Charles A. White of that place, in the Boston Journal of Natural History, (vol. vii, p. 209) he shows that out of a list of one hundred and two described species occurring in the Burlington limestone, fifteen commenced their existence in the beds below, referred by Prof. Hall to the Chemung; which as is well known, represents the Chouteau limestone of Prof. Swallow.

When we thus find these beds so intimately connected by the general affinities of their organic remains, as well as by the actual mingling of species with the undoubted Carboniferous rocks above, and bear in mind that the Chemung group in New York and Pennsylvania, is surmounted by another Devonian formation, (the Old Red Sandstone) between two and three thousand feet in thickness, the questions naturally suggest themselves,—are we right in referring these Chouteau beds to the horizon of the Chemung? Can it be possible that a great formation like the Old Red Sandstone, characterized by the remains of a peculiar fauna of its own, is wanting here between the Chouteau and Burlington limestones?

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* At the time Dr. Shumard described this crinoid, its exact geological position was not known, the specimen having been found loose at the base of a bluff composed in part of the Chouteau beds, and partly of the Burlington limestone. Its true position has since been determined to be in the Chouteau limestone, though it probably also ranges up into the Burlington limestone in some places.

But on the contrary, it may be said, if we do not adopt this conclusion, what are we to do with the fossils occurring in these rocks apparently identical with Chemung species? In reply to this latter question, we would state that although we have for a long time thought a few species occurring in the Chouteau beds identical with Chemung forms, later comparisons with New York specimens from the latter formation, lead us to doubt their actual specific identity. They are generally obscure casts of *Avicula* and other lamellibranchiate mollusca, some of which also resemble Carboniferous species. Admitting, however, that they are undistinguishable from Chemung species, does their presence here in the same beds with numerous other fossils totally distinct from Chemung forms, and not only more nearly allied to Carboniferous species, but in some instances identical with those occurring in acknowledged Carboniferous strata above, warrant us in placing these beds on a parallel with the Chemung group?

Mr. White, in discussing this question in his paper already cited above, says, "admitting that some of the species found in the lower beds have been identified with those of the Chemung group of New York, it settles beyond question their geological equivalency, but does not necessarily prove that they were contemporaneous. Indeed, it seems probable that they were not so, by an interval of time that it would take the species to migrate that distance. May it not, therefore, be inferred that the species originated at the east, and were migrating westward during the time that the bottom of the Chemung seas was sinking, and receiving upon it the deposit of the Old Red Sandstone,—thus making these Devonian rocks equivalent to the Chemung of New York, and contemporaneous, at least in part, with the Old Red of the Katskill Mountains?"

If any of the species found in the strata under consideration are really identical with Chemung forms, as has been supposed, Mr. White's suggestion that they had prolonged their existence by migrating westward, until long after the close of the Chemung epoch in New York, is not improbable, but in that case, we should not refer the rock in which they occur to the Chemung, but either to the Old Red, or to the Carboniferous, for in using any of these names we refer to a period in time, as well as to a group of strata. The entire group of fossils, however, found in these beds is far more nearly allied to the Carboniferous fauna than to that characterizing the Old Red either in this country or in the Old world; in short we know of nothing in this formation bearing any analogy to the fossils of the Old Red sandstone.

In conclusion, we would remark that the relations between the Chouteau and Burlington limestones in Missouri, Iowa, and Illinois, where both occur together, as well as the affinities of the fossils found in the former in the states mentioned, and at Rockford, Indiana, show that it should probably be referred to the Carboniferous system; or, at any rate that it is much more recent than the Chemung, and not equivalent to any New York rock.

It is not improbable, however, that the Portage and Chemung groups may be represented between the Chouteau limestone and the horizon of the Black Slate, at some places in the Western States alluded to, but if so, their presence there has yet to be clearly demonstrated.

Art. XIX.—*Chemical Contributions;* by M. Carey Lea.

1. *On the Preparation of Nitrate and Nitrite of Ethyl.*

NITRATE OF ETHYL.

The tendency of alcohol to decompose nitric acid is so great that while we have any number of processes for obtaining nitrite of ethyl, Millon's is the only one known for forming the nitrate. But Millon's process fails much oftener than it succeeds (such at least has been my experience) in consequence of the proportion of urea directed to be used being much too small.

Millon's process as quoted in Kolbe's Organische Chemie, directs to distill equal weights of alcohol and of colorless nitric acid sp. gr. 1·401. Not to operate upon larger quantities than 120 to 150 grammes, and to add a little urea, to the above quantity, at most ("höchstens") 2 grammes of urea.

Conducted as above, the operation succeeded with me but once out of four or five times. But by using a large quantity of urea, three or four times as much, or even more, success invariably follows, and there results a great saving of urea, because when the process fails, the materials, urea included, are entirely lost, whereas when conducted as just described, very little urea is consumed. It is only necessary to add fresh acid and alcohol, after the first proportion has been distilled to about one fifth, and the process may be repeated almost indefinitely, with the occasional addition of a little urea to keep up the proportion. The product in the second and subsequent operations is much larger than in the first.

Nor have I found it necessary to operate on the small quantities above mentioned, but have habitually used quantities amounting to 400—500 grammes, and should not hesitate to employ still larger ones. It is however more convenient to use the above quantity, and replace the material as fast as exhausted.

The facility with which nitrate of ethyl can be obtained by the above process must, I think, give it a decided advantage over the iodid of ethyl for the preparation of the ethyl bases, in the manner which I described in the last number of this Journal. The lower equivalent of nitric acid as compared with iodine is a matter of considerable importance, one hundred parts of iodine are capable of holding in combination something less than 23 of ethyl, whereas one hundred parts of nitric acid combine with over 53 of ethyl; a material point as the iodine on the one hand and the nitric acid on the other are merely the vehicles for bringing about the ethyl-substitution. In operations in pressure tubes where space is valuable, this is an important consideration; in an economical point of view also, the advantage appears to be greatly on the side of the nitrate of ethyl.

NITRITE OF ETHYL.

The action of nitric acid on alcohol in the preparation of nitrite of ethyl may be conveniently moderated by protosulphate of iron, and the nitric acid reduced by that agent instead of at the expense of part of the alcohol. The proportions which I have used are, nitric acid sp. gr. 1·37, 90 cc., alcohol of 90 per cent, 150 cc., ferrous sulphate 45 grammes. The product is very large, the distillate on being shaken up with water yields more than half its bulk of nitrite of ethyl. The latter is not perfectly free from aldehyd, but more so than that obtained by some other processes. The action is perfectly quiet to the last, and the distillation may be carried on rapidly.

2. *On the Effects of Reducing Agents upon Nitrite of Ethyl.*

As ammonia under the influence of oxydizing agents is burned to nitrous and nitric acids, so these acids when acted upon by reducing agents, might naturally produce ammonia, or in the presence of a decomposing ethyl compound, might give rise to the formation of substituted ammonias. The experiment however seems to prove that while ammonia is a constant result of this reduction, it varies greatly in quantity according to the particular reducing agent employed, and that substituted ammonias are not produced, at least not in recognizable quantities. The following were the reactions observed:—

Reduction by protochlorid of tin.—Alcoholic solution of protochlorid of tin added to nitrite of ethyl causes violent effervescence, but no disengagement of nitrous fumes. The solution of protochlorid was added until reaction ceased. The whole was then distilled with caustic potash and the resulting gases conducted into dilute chlorhydric acid, which by evaporation left only a film of substance—this gave on examination distinct indications of ammonia.

Reduction by sulphydric acid.—A large quantity of sulphydric acid was conducted through nitrite of ethyl. Slight effervescence was produced and much sulphur deposited. The liquid filtered from the sulphur proved to contain a large quantity of ammonia, but no ethylamine, di-, or tri-ethylamine, although the mode of examination employed would have detected a very small quantity if present. After the removal of the ammonia, the solution heated with $Pt\,Cl_2$ gave a small quantity of a brown precipitate of a platinum salt, which when heated glowed for a long time showing the presence of much carbon, and finally left a residue of reduced platinum. The amount obtained was too small for investigation, but 20 or 25 milligrammes of Pt. salt were obtained from the decomposition products of 50 grammes of nitrite of ethyl.

This result therefore, as far as to the production of ammonia, corresponds with that obtained by M. Emile Kopp,* although, as that chemist employed sulphydrate of ammonia instead of sulphydric acid, it does not appear that the production of ammonia by the reaction itself, could be positively established.

Reduction by ferrous acetate.—Nitrite of ethyl was added to about three times its bulk of ordinary acetic acid, and sufficient alcohol to make them mix. To this mixture iron filings were added. A powerful action set in without aid of heat. Deutoxyd of nitrogen was evolved in considerable quantity. When action had ceased, the resulting liquid was filtered, evaporated with chlorhydric acid and distilled with caustic potash. The distillate contained only traces of ammonia.

The effect of the reducing agents on nitrite of ethyl is therefore very different. Only with sulphydric acid was any considerable quantity of ammonia produced, and only with ferrous acetate was deutoxyd of nitrogen evolved.

3. *On the Preparation of Urea from Ferrocyanid of Potassium.*

In the preparation of urea by Wöhler's method the amount obtained always falls short of that which should theoretically be produced. Having remarked that even when the operation is carefully performed with Liebig's proportions, the lixiviate always contains undecomposed cyanid of potassium, it occurred to me to try the effect of a more thorough oxydation, and the result proved very favorable. The following was the course adopted.

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* Gerhardt, *Chimie Org.*, ii, 347.

Of roasted ferrocyanid of potassium 850 grammes are mixed with 318 grammes of thoroughly dried carbonate of potash in grains, and fused in an iron vessel. When decomposition is complete, the vessel is a little cooled and 1900 grammes of red lead are to be added, not all at once, but 300 to 400 grammes at a time, with intervals of 10 minutes, stirring and keeping up heat enough to retain the whole in a state of fusion.

After the last addition of red lead the mixture is suffered to remain half an hour on the fire to complete the reaction. Heat is applied in all for about four hours. In this way the cyanid of potassium becomes thoroughly oxydized. The process is concluded in the usual manner, and 500 grammes of urea are obtained. No particular precaution is necessary during the fusion, or even the lixiviation with cold water, but in evaporating the solutions the greatest care must be taken to carry off the vapors.*

Philadelphia, April, 1861.

ART. XX.—*Contributions to the History of Picric Acid;* by M. CAREY LEA, Philadelphia.

Solubility in Sulphuric Acid.

IT is stated in the text-books that picric acid is insoluble in sulphuric acid. It is however soluble to a small degree in strong sulphuric acid; in a more dilute acid it is apparently wholly insoluble, until the dilution reaches a certain point when it increases again. If picric acid be left in contact with oil of vitriol, and the latter be decanted, and mixed with two or three times its volume of water, the picric acid is deposited on cooling in what appear to be very minute square or nearly square scales.

Picric acid crystallizes in the rhombic system, and if we suppose these scales to be formed by predominating $\infty \bar{P} \infty$ planes bounded at the edges by octahedral planes, they should be rhombs approaching very nearly to squares, having their axes as ·9374 : 1·0000.

If cold saturated aqueous solution of picric acid be mixed with sulphuric acid diluted with an equal volume of water, the following results are obtained:

1 vol. solution of picrate, 4 vols. dilute sulphuric acid. (1 vol. acid, 2 vols. water.)	No precipitate, solution remaining as colorless as water.

* For continuation of Mr. Lea's "Contributions," see p. 210.

1 vol. sol. picric acid, 2 dilute sulphuric acid, same dilution.	No precipitate, solution very nearly colorless, faintest tinge of yellow only visible.
2 vols. solution picric acid, 1 vol. sulphuric acid, same dilution.	Nearly the whole of the picric acid was precipitated.

The amount remaining in solution continued in further trials to diminish as the sulphuric acid became more dilute, until a maximum was reached with

3 vols. solution picric acid, 1 vol. dilute sulphuric acid, same dilution.	

It thus appears that mixtures of sulphuric acid and water reach their minimum of solvent power for picric acid when the mixture consists of about 1 vol. acid to 11 vols. water. The proportion of water may be still further increased without materially increasing the solvent power for picric acid. If a cold saturated solution of picric acid be mixed with even but $\frac{1}{17}$ of its volume of sulphuric acid, almost the whole is thrown down.

The fact that the characteristic color of picric acid, which it maintains so persistently through all its combinations, and which is so powerful that, as I have found by actual experiment, a milligramm will distinctly tinge a kilogramm of water, or in other words, that water is colored by one millionth of its weight of picric acid—the fact that this color is totally destroyed by sulphuric acid of a certain strength, without in any way decomposing the acid, is very remarkable. Four volumes of sulphuric acid diluted with five volumes of water exhibit this property, and picric acid dissolves in such a mixture to a colorless solution. This peculiar property has no doubt led to the supposition of the insolubility of picric acid in sulphuric acid above referred to.

Water containing $\frac{1}{10.000}$ of picric acid exhibits a bright yellow color. With $\frac{1}{300.000}$ the color is still distinct, even in a stratum of not over an inch in thickness. But in large quantities a millionth gives a distinct color as above mentioned.

Tests for Picric Acid.

The best tests for picric acid are

Ammoniacal solution of sulphate of copper, which gives a greenish crystalline precipitate.

Alkaline sulphid with excess of alkali, which with heat gives a deep red liquid.

Alkaline cyanid with ammonia, which when heated gives also a red liquid.

The following table will exhibit the relative sensibility of these reagents:

Strength of aqueous solution of picric acid.	Ammonio sulphate of copper.	Potash liver of sulphur (heat.)	Cyanid of potassium.
$\frac{1}{1000}$	Immediate precipitate.	Solution becomes sherry wine red.	Pale sherry red, with heat becoming deep red.
$\frac{1}{4000}$	No precipitate at first, but by standing a few minutes a distinct one.	Deep yellow with a tinge of sherry color.	Deep yellow, tinge of sherry color, deepened by heating.
$\frac{1}{5000}$	Distinct precip. by standing.		
$\frac{1}{10000}$	No precipitate.	The yellow color was slightly deepened, the cyanid test is the more delicate of the two.	

Purification of Picric Acid.

Since my former observations on the purification of picric acid,* I have had occasion to prepare considerable quantities of the acid for my examinations and find that all purifications by converting into potash salt are inapplicable except for very small quantities. The picrate of potash crystallizes out by so small a fall of temperature that the filters, even when kept heated by a double funnel become immediately clogged, and the operation becomes to the last degree tedious and troublesome. As the picrate of lime is very soluble, it seemed probable that it might afford a convenient means of solution; it has indeed been already recommended for that purpose.† But I find it wholly inadmissible. A basic salt is formed which falls to the bottom with the excess of hydrate of lime, and great waste ensues. The insolubility of alkaline picrates in cold alkaline solutions which I have described in a previous number of this Journal, furnished me with an excellent process. The crude acid is saturated with carbonate of soda, an excess of which is to be avoided as it tends to dissolve resinous matter. The hot solution is then easily filtered, and into the filtrate a few clear crystals of carbonate of soda are placed. On cooling, the picrate of soda crystallizes out almost as completely as the potash salt would have done, and all the wearisome delay in filtration is avoided. From the mother water more picric acid may be recovered by the addition of a little carbonate of potash. In decomposing alkaline picrates to separate the acid sulphuric, (and not as usually recommended, chlorhydric) acid should be used, because a moderate excess of sulphuric acid throws down a great portion of the acid which would otherwise remain in the mother water. A moderate but decided excess of acid is absolutely necessary, because otherwise a portion of alcaline picrate escapes decomposition. Even then, it is advisable to recrystallize the acid from alcohol. If picric acid be dissolved by the aid of heat in a solution of sulphate, nitrate, or almost any salt of potash, more or less picrate of potash will crystallize out on cooling. I have thought this process not devoid of interest, because picric must become more extensively known in the laboratory and in the arts than hitherto.

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* This Journal, Nov., 1858.

† See Gmelin, Eng. Ed., vol. xi, p. 214.

Effect of Reducing Agents.

The effects of reducing agents when alkali is not present, or not present in excess, (in presence of excess of alkali, picramic acid is formed), are very variable, depending upon slight differences which it is very difficult to seize. I subjoin some of the best marked results obtained.

A mixture of picric acid, alcohol, iron filings, and acetic acid, were digested for an hour at a heat a little below 212°. The filtrate was intensely blue, by standing for half an hour or less, became brown and muddy, depositing a blackish powder, in small quantity, and without trace of crystallization. This filtrate was not changed in color by acids, or apparently affected by them. Alkalies decolorized it. Its shade of blue varied considerably in different experiments, sometimes full blue, sometimes violet, sometimes greenish.

Other experiments were made by acting on picric acid by zinc and dilute sulphuric acid. After an action of some hours, the solution was mixed with alcohol and filtered. The filtrate heated with bicarbonate of potash in successive portions, gave a fine violet liquid, which with further addition of alkali became deep blue with a tinge of violet. According as acid or alkali were present in excess there was more of the violet or blue shade. The colors were always very fugitive, and changed to dirty brown by standing, with deposit of an amorphous blackish powder, (very small in quantity compared with the picric acid used,) which was soluble in acids, and insoluble in alcalies.

These experiments although many times repeated did not lead to the isolation of any substance of interest. There is a certain amount of resemblance between these reactions, and those of some of the decomposition products of phenylamine: the latter contains the radical $C_{12}H_5$ which exists in a substituted form in picric acid.

Philadelphia, April, 1861.

ART. XXI.—*On the Relation between our Perception of Distance and Color*; by Prof. O. N. ROOD.

THE fact that a landscape appears more vivid in color, when viewed by the eyes brought into an abnormal position, as in looking under the arm, &c., is well known.

Some persons have attempted to explain this fact by the influence of an augmented pressure of the blood upon the retina.

In an easy reclining posture, where such pressure can hardly exist, I observe this heightening of tints with great distinctness, also by viewing the inverted image of the landscape by total reflexion through a rectangular prism, the head being in its natural position.

Dr. A. Müller* with more probability has referred this appearance to the different accommodation of the eye for horizontal and vertical lines.

To me it seems that this effect is intimately connected with our perception or non-perception of distance. In gazing at landscapes, the ordinary habit of most persons, artists excepted, leads them to pay attention to the forms and *distances*, (which alone have a practical value as objects of observation), and to neglect the *color*, particularly those portions of it which are subdued. When now by any means the mind is prevented from dwelling on distance, it is thrown back on the remaining element, color; and the landscape appears like a mass of beautiful patches of color heaped upon each other, and situated more or less in a vertical plane.

(1.) A perpendicular position of the eyes reduces very considerably our perception of depth or distance, so that false estimates of it are formed by the eyes in this new situation. With the exception of objects in the foreground, all things seem to lie not far removed from the same vertical plane.

The reason is partly to be found in the fact, that while in normal vision our binocular perception of depth is obtained by regarding vertical lines, trees, &c., in vertical vision the same objects, though instinctively sought afford us no information.

(2.) In normal vision with a single eye, there is certainly, in a binocular sense no perception of depth, nevertheless the mind occupies itself with the idea of distance, and if the objects are familiar there is no augmentation of color perceived. By inverting the image of the landscape with a rectangular prism the objects fall into almost one plane, are diminished in apparent magnitude, and the mind unable to trace distances through this maze, is forced to dwell on the mass of tints presented.

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* Pogg., vol. lxxxvi, p. 147.

(3.) With the erecting or inverting telescope, in proportion as the objects viewed are divested of the idea of solidity or depth, can their more delicate tints be perceived. Objects, which in normal vision seem to us nearly without color, are best fitted for these observations; a bare pile of stones and dry mud viewed through a telescope appears often like a richly tinted water color drawing.

It would seem probable that if we could add to paintings of landscapes the element of distance, the mind occupied with this would no longer dwell on the richness of the tints. In confirmation, I find that colored stereographs of landscapes, which out of the stereoscope seem exaggerated in tint, when placed in the instrument no longer appear too highly colored.

From the foregoing considerations, then, it would appear that when the mind is engaged with the perception of distance, the *presence* of color is often overlooked; its *absence* may remain unnoticed from the same cause, for in uncolored stereographs of objects that are perfectly familiar to the observer, it will sometimes be noticed, that those articles which do not greatly differ in color from the tint of the photographic paper, are seen in the stereoscope with an approximation to their natural hues; upon withdrawing the slide from the instrument no trace of such tint is perceived. Objects that are free from lustre, as well known carpets, answer for this purpose. That this should be the case with the tinted photographic representations of white objects can be explained of course in another way.

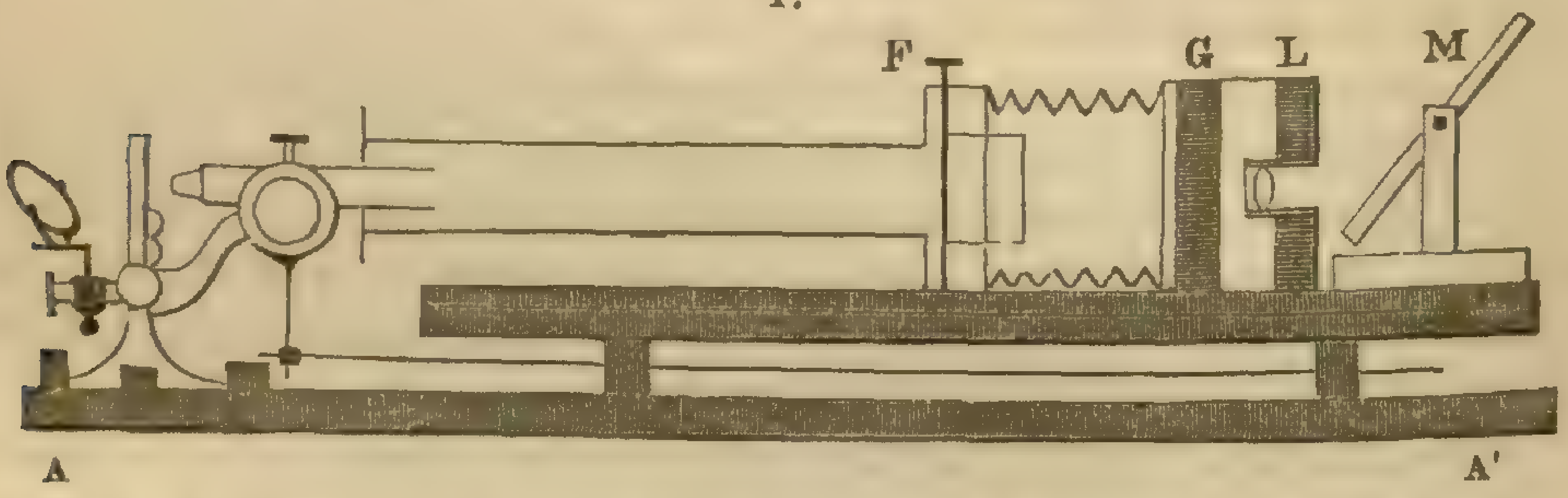
[The chromatic effects here noticed by Prof. Rood are well seen in the ordinary camera and dark chamber. Thus the human countenance when not florid, presents to the unartistic eye few or no traces of pink or flesh color—but every one who has seen it in the camera must have observed with what distinctness the image is colored. The same is true of familiar landscapes, when seen inverted upon the screen in a dark chamber. Here the neutral tints which in nature are almost unnoticed by the common observer, stand out as distinct patches of color in the way so well described by Prof. Rood.—S.]

ART. XXII.—*On the Practical Application of Photography to the Microscope;* by Prof. O. N. ROOD, of Troy, N. Y.

WHILE the value of the photographic delineation of microscopic objects, as a means of accurately recording observations, seems to be generally acknowledged, yet owing to the real or imaginary difficulties with which the process is beset, but very few working microscopists have adopted it.* After eight months of steady experimental work on the subject, this fact appears to me a matter of astonishment, for the difficulties which are not inherent, mostly disappear when proper precautions are taken. I propose to mention briefly certain points in my experience, and to indicate the methods pursued.

Arrangement of the Apparatus.—The microscope is brought into a horizontal position, and connected with a camera box by a blackened paste board tube; much vexation will be avoided by constructing at the outset the arrangement seen in the wood cut, fig. 1. Blocks are fitted around the foot of the microscope, that it may be firmly held in position, and the camera box slides between parallel strips of board, so that its distance from the microscope can be varied. The length of A A′ is seven feet. The frame holding the ground glass slides in at G; behind it at L, is a door on hinges carrying an achromatic lens of two inches focal length, for the purpose of magnifying the image on the ground glass while focusing; the glass plate should be finely ground. A tube lined with black velvet is to be inserted

1.



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* In Vienna microscopic photographs have been produced under the direction of AUER. POHL and WESELSKY have also worked at this subject. (*Sitzungsbericht d. Kais. Akad*, 1857, xxiii, vol. 1, page 317); at an earlier date MAYER of Frankfort obtained fine photographs of this kind. BERTSCH presented similar results to the French Academy (*Comptes Rendus*, 1857, xliv). NACHET also obtained good results. HODGSON (*Quart Journ. of Micros Science*, 1853, ii, p. 147), DELVES, (3d No. of the same, p. 57), SHADBOLT (ibid, p. 165), HUXLEY (ibid, p. 178 also No. 4, p. 305), WHENAM (same Journal, 1855, No 10, p. 1), and KINGSLEY (*Phil. Mag.*, 1853, June, p. 461), have published accounts of their more or less successful results. HARTING on the Microscope, *Braunschweig*, 1859. To the above must be added the great work now being issued in numbers in Munich entitled *Atlas der allgeminen theirischen Gewebelehre, herausgegeben von* TH. V. HESSLING *und* J. KOHLMANN, *nach der Natur photographist von* JOS. ALBERT.

in the compound body, as recommended by Shadbolt, if the eye-piece is not employed. Precautions must of course be taken that light does not enter at unguarded points. At F is a rod connected with a flap of blackened sheet brass in the interior of the box, with which the exposure of the sensitive plate is very conveniently effected. It is obvious that while the operator is manipulating the mirror, or using the stage movements, on account of the length of the apparatus it is impossible for him to see the ground glass, or even to know when light has been thrown on it; a plane mirror, mounted as seen at M, reflects the image of the ground glass, enabling him not only to arrange the illumination with nicety, but to select the microscopic object, and to focus on it approximately. While the mirror is in use the door carrying the achromatic lens stands open: the mirror is afterwards removed and the focal adjustment completed with the help of the lens, by the rod and lever attached to the rack work of the microscope. If the rack work is moderately good, this arrangement is very delicate. When a high magnifying power is employed, it is essential that the microscope be provided with stage-movements to bring the object into its proper position. The lever stage is not to be recommended for this purpose.

Illumination of the object.—That direct sunlight is greatly to be preferred is admitted by those who have experimented on this point. With light from a white cloud I have obtained negatives, in from one to three minutes, with 1 inch and ⅛ inch objectives; though not highly magnified, they were inferior to those taken with sunlight. Shadbolt obtained negatives by concentrating the light of a small camphene lamp on the object with two lenses. Whenam in repeating this experiment met with no success. I concentrated the light of two flames of a "burning fluid" lamp with a bulls-eye-condenser on the object, employing the 1 inch objective without an eye-piece, and obtained, with several samples of collodion from different manufactures, absolutely no image at all, after an exposure of five minutes. With samples prepared by myself tolerably intense negatives were obtained in four minutes. With the ⅛ inch objective a faint image was obtained in the same time. For my own work direct sunlight is always employed.

It is well known that the proper display of microscopic objects, requires that great attention should be given to their illumination, not only in degree, but in kind; much has been written on this subject and an astonishing amount of labor bestowed on it. All this applies with double force to the illumination preparatory to the introduction of the sensitive plate; refined methods here find a most useful application. If the power employed be under 100 diam., the plane or concave mirror will answer, if the stage be provided with a diaphragm-plate having apertures of different size. The mirror should be most carefully adjusted, so that the maximum of distinctness in the image on the ground glass is obtained. I was kindly furnished by Prof. Chas. A. Joy with a *silvered* mirror which Liebig presented to him some months ago while on a visit in Europe. It furnished brighter light than the ordinary amalgam mirror; the use of Liebig's mirrors for this purpose, as well as for ordinary microscopic work is to be recommended. With powers from 100 to 2000 diameters, a condenser of some form is needed. For powers from 100 to 400 diameters, an achromatic condenser adjustable by rack work was used. Such a condenser must be provided with a series of diaphragms having circular apertures differing in size, also with a set of central stops for annular oblique illumination.

Trial alone will settle which aperture gives the clearest image in any particular case. As the lenses of this condenser were not large, I constructed for powers from 400 to 2000 diameters a Wollaston doublet, with an angular aperture of 44°, the lenses being ·5 and ·6 of an inch in diameter. This condenser when provided with a similar set of diaphragms was found to answer very well, both as to the degree and quality of the light, negatives being obtained in 15 seconds enlarged 1500 diameters. As the chromatic aberration was not corrected, it was found easy to illuminate the object either with white or bluish-white light, use of the red or yellow rays being of course carefully avoided.

The proper distance of the condenser from the object is a point of much importance, and is best ascertained by carefully repeated trials. To obtain really good results much nicety in arranging the illumination is required: this is a matter in which microscopists are well practiced, but to secure the best results possible *under the circumstances*, as in photographing test objects, the art and patience of the operator is taxed to the utmost, and several days are often consumed before a really satisfactory result is attained, even in the case of a single object.

Focal adjustment, &c.—Trouble will be saved by selecting the exact object which is to be photographed, by the microscope in an upright position, the instrument is then inclined and connected with the camera. After the compound body is thus placed, if the objective is provided with a "screw collar" for correction, this adjustment must be *carefully made.* Even when this point has received attention, it by no means follows that the chemical focus coincides with the visual, and the exact correction necessitated by this difference must be ascertained by trial. This can be effected by the use of the fine adjustment, (see Shadbolt's paper). Contrary to some other observers, I have found it necessary when using sunlight, with both high and low powers, with, and without eye-pieces, to make this correction carefully. The use of the rod and the lever and achromatic lens has already been mentioned. After the corrected image has been thrown on the ground glass it will remain nearly unaltered from 30 seconds to 10 minutes, according to the power and mode of illumination employed.

Collodion.—This article when furnished by makers of repute can of course be used, though it is better for more than one reason to be independent of the dealers if possible. A considerable number of samples of pyroxyline were prepared according to different receipts and sensitized variously. The very simple process described by Waldack, on page 266 of his treatise on Photography, was found with slight modifications to yield an excellent article.* The strength of the sulphuric acid was slightly greater than recommended by him, no water was added, the temperature also was slightly higher, at the time of the immersion of the cotton: a more prolonged washing than that prescribed in this work is desirable. This collodion can be sensitized with advantage by the iodid and bromid of cadmium in the proportion of four to one. A receipt published in Humphrey's Photographic Journal has lately been used by me with very good results.†

No. 1.	No. 2.
Plain collodion, 1 oz.	Plain collodion, 1 oz.
Iodid of ammonium, 5 grains.	Iodid of potassium, 5 gr.
Bromid of potassium, 3 do.	Bromid of ammonium, 3 gr.

Dissolve the iodid of ammonium and bromid of ammonium in alcohol, the iodid of potassium and bromid of potassium in the least possible quantity of water, before adding them to the plain collodion. Mix Nos. 1 and 2 in equal parts for use.

This collodion when used according to the wet process, though not very intense when first made, is quite sensitive, negatives of landscapes being obtained in ¼ of a second indicating by their strength that a shorter time would suffice. It acquires intensity by keeping. The exposure is effected by the flap at F, and will last from ¼ of a second to 4 minutes according to circumstances. The development is as usual, hyposulphite of soda being used as a fixing agent. The use of the bromid of arsenic, mentioned in the same Journal, gave with some samples of collodion, excellent results so far as intensity was concerned.

The negatives thus obtained, are examined by a lens of one inch focal length, to test their degree of sharpness. This quality will not only vary with the manipulation, but with the nature of the object; the sharpest negatives obtained by me, when examined by a power of 40 diameters, appear as well defined as finely executed lithographs seen by the naked eye, while other classes of objects, (dots in pine wood, &c.,) with all care, yield negatives which present the same appearance under a much lower magnifying power.

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* Treatise on Photography by Chas. Waldack, Cincinnati, 1860.

† Humphrey's Journal, Joseph H. Ladd, New York.

Positive prints.—In order to preserve the fine details, the prints should be taken on glass, not on paper; mica answers when a print is to be transmitted by mail. Great care should be used that little or none of the fine markings on the negative are lost in this process; a bright light (sunlight thrown on the negative backed by ground glass,) a small diaphragm before the copying lens, and careful allowance for the chemical focus, are the essentials. To produce enlarged positive prints on glass, the negative is placed on the stage of the microscope and treated like a microscopic object, the magnifying power varying from 5 to 20 diameters. If the prints are to be on *paper*, it will be found that a more liberal use of nitrate of silver and chlorid of gold, than is generally recommended makes success easy.

Magnifying powers employed.—To produce enlarged images the objectives as is well known may be used alone or in connexion with an eye-piece. In the former case with proper illumination, sharp images are produced when the distance between the object (on the stage,) and the ground glass is as great as five feet. With this distance the

1	inch	enlarges	65	diameters.
$\frac{1}{3}$	"	"	190	"
$\frac{1}{7}$	"	"	460	"

In using the objectives in this way the screw collar is set *after* the microscope is connected with the camera.

For more highly enlarged images it is best to add the long eye-piece as has been practiced by some experimenters. The adjustment of the screw collar can then be very nearly completed before the microscope is connected with the camera, which is a great saving of time, it will of course fall nearer the mark "uncovered" than in the first case. However perfectly this operation may be performed in either instance, allowance must still be made for the actinic focus. By varying the distance between the eye-piece and the ground glass different degrees of enlargement are obtained. When the long, or two inch eye-piece, is used, the distance from the object-slide to the eye *lens* being 12 inches, from the latter to the ground glass 34 inches, then

1	inch	enlarges	160	diameters.
$\frac{1}{3}$	"	"	550	"
$\frac{1}{7}$	"	"	1300	"

Powers obtained in this way with the two latter objectives have been used by me with advantage.

Thus with the $\frac{1}{7}$th 113° aperture, the Wollaston doublet of 44° aperture having a central stop, being used as a condenser, I obtained sharp negatives of the *P. angulatum* magnified

1300 diameters with well defined hexagonal markings similar to those obtained by Whenam with a $\frac{1}{12}$th of 130° aperture. Portions of the negative bore a photographic enlargement of 10 diameters. Mr. Whenam announces* that he has discovered by the use of a $\frac{1}{50}$th of large aperture, made by himself, that the markings on this object and on some others, are really due to spherical particles of quartz which can be made by illumination to appear hexagonal. With a power too low, I obtained photographs of the *P. Balticum* with *hexagonal* markings; with a higher power and larger angle of aperture the tendency was to the spherical form.

Photographs by Polarized light.—A Nicol's prism is placed under the stage, one also directly behind the objective, sunlight is reflected from the mirror, and one of the prisms revolved till the field is dark; with the low powers by this simple arrangement photographs of objects may be obtained, which exhibit the structure revealed by the polarized light. For higher powers it is necessary to use the polarizing arrangement described by von MOHL, *Pogg.*, vol. cviii, p. 178, and recommended by Carpenter; that is, the light from a large Nicol's prism is concentrated on the object by an achromatic condenser. The perfection with which this apparatus operates may be inferred, when I state that photographs of the cross and rings in starch granules as well as of the *P. angulatum* in a dark field, were obtained by me without difficulty. Von Mohl remarks, that with inferior apparatus some very distinguished observers have been unable even *to see* these appearances. The selenite stage can of course be used when it is found desirable.

By arranging the apparatus according to the plan adopted by Prof. v. Kobell in his micro-stauroscope, (this Journal, [2], vol. xix, p. 425), the peculiar effects which microscopic crystals produce on the cross and rings of calc spar, can be photographed. By removing the condenser and objective, as well as the slide containing the crystals, beautiful photographs can be obtained of the normal cross and rings; the systems of rings in other crystals can be photographed by substituting them in place of the calc spar, as well as the changes which they undergo by combination with plates of doubly refracting substances, (circular analysis, &c.,) it being merely necessary to introduce the plates or films at the proper positions. I was shown by Prof. Dove some years ago while in Berlin, photographs of the normal cross and rings around the axis of the calc spar, but so far as I know this is the first attempt to photograph the changes which the cross and rings undergo by the action of microscopic crystals.

Stereoscopic photographs of microscopic objects, can be obtained with the monocular microscope, by covering first the right half of the objective, then the left by a suitable brass cap, and taking two successive pictures. When using this method it becomes necessary to move the mirror towards the right or left hand with each successive exposure, which is not only inconvenient, but often produces a slight distortion, that prevents the proper stereoscopic union of the two photographs. On this account I have generally adopted a different plan; the object is placed on an extra stage, which can be inclined from 5° to 10° as seen in profile in the wood cut, fig. 2; it is photographed first at one angle then at the other. In practice the manipulation is easy, and no particular difficulty is experienced from the fact that the extreme right and left hand portions of the field are thrown slightly out of focus. High and low powers can be used equally well. The second negative should be taken immediately after the first, before the illumination has altered. I do not know that stereographs of microscopic objects have actually been taken by other experimenters, though this may easily be the case.

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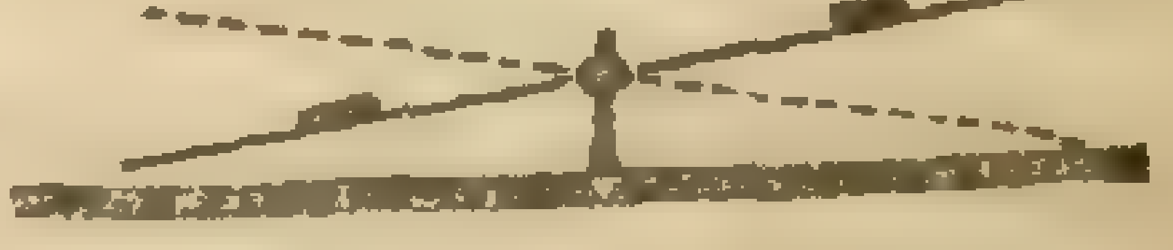
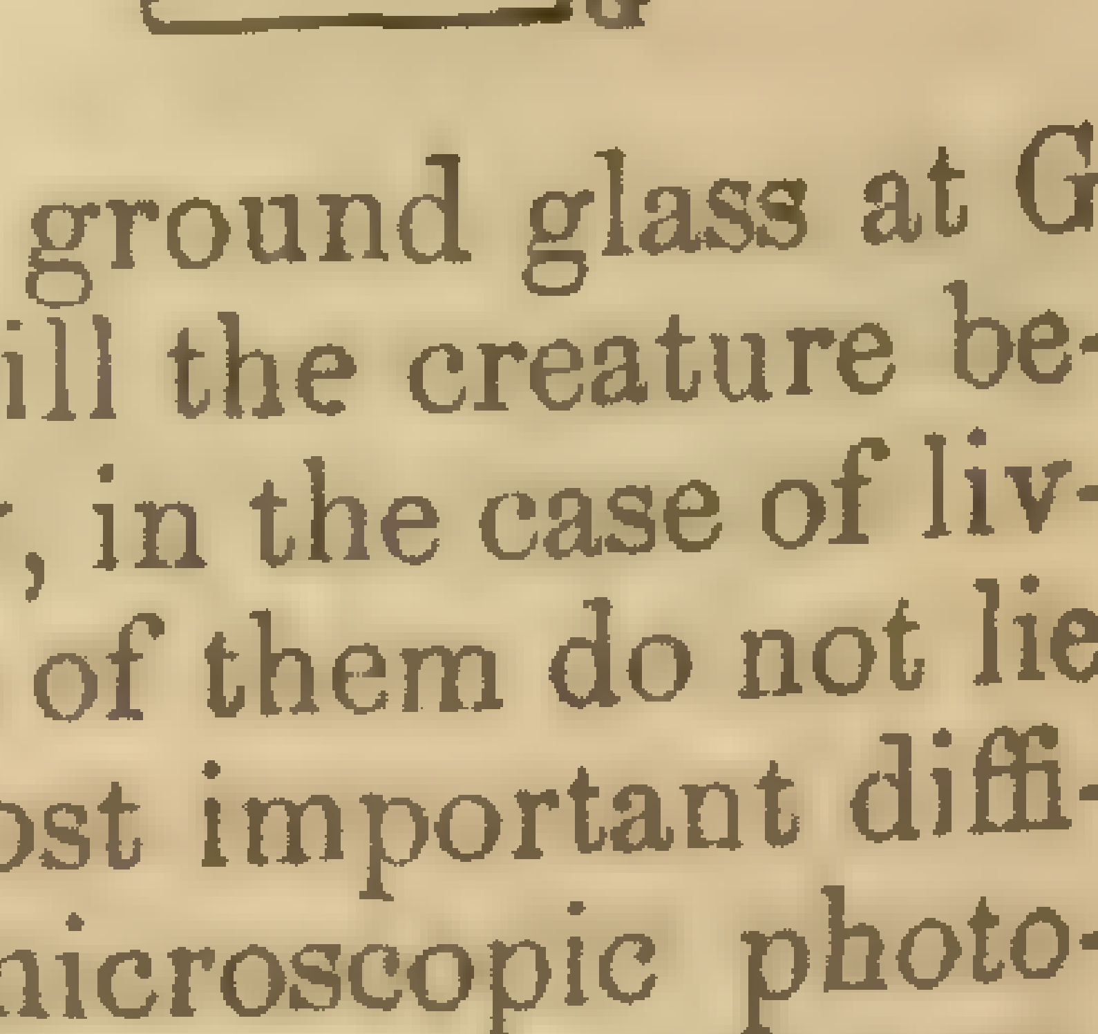
* Quart. Journal of Mic. Science, No. xxxi, p. 145.

2.



Living organisms offer the photographer some difficulties by their constant motion about the field, and in and out of the focus. It becomes necessary to adopt a plan by which the image can be thrown on the sensitive plate, the very instant after the animalcule has been brought into focus. The following method has been used by me with success to obviate this particular difficulty: a plate of glass with parallel sides is introduced at an angle of 45° into the tube outside of the camera; it reflects an image of the object to the ground glass at G, fig. 3, which is placed so that an equally sharp image of the same object is formed at G′. The sensitive plate is introduced at G′, the flap at F being closed; with one hand the operator, by the aid of the image at G, focuses on the animalcule, just as this is effected, the plate is exposed by the other hand turning the flap. If the collodion is sensitive, a second, or less, suffices to give an image; if a longer exposure be desired the image of the animalcule on the ground glass at G can be watched, and the exposure prolonged till the creature begins to change its position. The real difficulty, in the case of living organisms, is found in the fact that all parts of them do not lie in the same focus; this in fact is one of the most important difficulties connected with the whole subject of microscopic photography. But the introduction of a slight modification in the ordinary compressorium, removes it in many cases; the plate of glass on which the objects rest, instead of being plane, is made slightly *convex*, by the use of a spectacle lens of rather long focus; objects to be examined are placed near the point of contact, and pressure applied as usual, when they are brought nearly into the same plane.

3.



Photographs of opaque objects were obtained by concentrating sunlight on the object, either with the concave mirror properly mounted, or with the plane mirror and bulls-eye condenser. The $1\frac{1}{2}$, 1 and $\frac{1}{3}$ inch objectives were employed. The color of microscopic injections for this purpose should be blue or white, though with a long exposure photographs were obtained of yellow injections.

I am indebted to Dr. Wolcott Gibbs and to G. T. Strong, Esq., for valuable suggestions relating to the subject of this article.

As some interest has lately been manifested with regard to eye-pieces, it may be proper to state, that, in the course of this investigation, three eye-pieces were constructed, No. 1, on the general Huyghenian plan, the eye lens being an under-corrected *achromatic*;[*] the distance between the eye and field lens could be varied at pleasure, as advised by Amici; as a single microscope its power was eight diameters. The general performance of this eye-piece seemed to me somewhat better than that of the plain Huyghenian eye-pieces with which it was compared.

No. 2 was a Kellner eye-piece, the distance between the lenses could be varied; alone it enlarged 12 diameters. The performance was good. No. 3, consisted of two under-corrected achromatic lenses combined in the Huyghenian manner; alone it magnified 25 diameters. When used as the eye-piece of a telescope it gave a pretty good image; as a microscopic eye-piece, it was inferior to No. 2 in spite of its superior magnifying power, except perhaps when used with well corrected objectives of large angular aperture. With all three eye pieces the correction of the objectives remained unaltered in kind.[†]

Troy, N. Y., July, 1st, 1861.

ART. XXIII.—*On some questions concerning the Coal Formations of North America.* (Continued from p. 25 of this vol.)

By LEO LESQUEREUX.

Families, Genera and Species of Fossil Coal Plants in the United States.

This general examination is made for a two-fold purpose. First; to see how the remains of fossil plants found in our American coal-measures agree in characters with those of Europe: or rather to find if these remains of ours cannot give some farther light concerning species, genera and families of coal plants, already known from European researches. The fossil flora is very similar to the history of olden times. It is known only by fragments and these must be connected by links and repeatedly elucidated by corresponding data, collected from the monuments of various nations.

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[*] The orthoscopic eye-piece of Grunow consists of an eye lens partially achromatized in combination with a field lens differing in form from the ordinary Huyghenian.—O. N. R.

[†] After this article was in print, I received from a friend a paper of AMASA M. EATON, Esq. (*Proceedings B. S. N. H.*, vol, viii, p. 105.) Mr. Eaton has devoted himself with success to the production of *ambrotypes* of microscopic objects.

Troy, N. Y., July 23d, 1861. O. N. R.

Second; to present a comprehensive review of the main facts known up to the present time about our coal flora and to establish, by a critical comparison, the essential characters of the families, genera, and even some species of our coal plants.

§ 1. *Fucoides*, Brgt.

Under this name, a number of vegetable remains of the coal have been formerly described and referred to marine plants. Indeed some geologists have applied the term *Fucoides* to every one of those uncertain forms, mostly stems and roots, which could not be referred to some species known and published before. The *Fucoides* are not only very rare in the true coal measures, but I even doubt if a single specimen of a true marine plant has ever been found in these measures. At least I have seen none, and all the numerous specimens sent to me under that name were incomplete fragments, either of stems or of roots, of some true coal plants.* Mr. Brongniart had already remarked that all the vegetable filaments considered as *Confervites*, like those published by Artis under the name of *Hydatica* and *Myriophyllites*, were roots. Mr. Unger in his classification of fossil plants according to geological formations, (*Genera and species*, page 533), quotes among *Fucoides* of the coal: 1st. *Chondrites Prestvici* (Mor. cat.) an undescribed and unknown species, whose locality is not even mentioned. 2d. *Chondrites dissimilis* (Eichw.) from the mountains of Donetzky in Russia. It is published by Eichwald in the Urw. Russia, p. 89, t. 3, fig. 3, but rather resembles a *Hymenophyllites* than a Fucoid. 3. *Chondrites trichomanoides* Göpp., *Systema foss.*, t. 30, fig. 26, a plant formerly described by the author as a *Trichomanites* and which appears to belong to that section of Hymenophyllites which I have called *Pachyphyllum* (Penn. Report). This plant was found in a kind of shale called by Mr. Göppert *stinkende* with remains of fishes. 4 and 5. *Fucoides Alleghaniensis* (Harl.) and *Fucoides Brongniarti* (Harl.), both belonging to the Silurian or Medina sandstone of our country. 6. *Rhodomelites bijugus* (Eichw.) from the mountains of Russia with *Fucoides subtilis* and *Fucoides tæniola* of the same author. These two last species are not described. Though Eichwald is a very good observer, we can but suppose: either that his four species of fucoides are fragments of coal plants, or that the formation where they have been found is not within the true coal formations. In his *Tableau des Genres*, Mr. Brongniart mentions as pertaining to the *Carboniferous period* two *Chondrites* and two *Amansites*. These last two are *A. serra* and *A. dentata* belonging to the Silurian of Canada; the two others also pertain to the transition formations inferior to the Devonian.

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* We cannot call them either marine or fresh water plants. The coal plants as remarked before, had a peculiar nature, like those of the peat bogs and were appropriated to the formation of the coal. They could vegetate with slight modification of forms either in or out of water and probably both in marine and fresh water.

I could easily refer to *Fucoides* a number of specimens undeterminable, branches or stems that look like marine plants. I have especially a smooth, flexuous, linear, apparently rounded branch about the fifth of an inch broad, perfectly equal in its whole length, resembling a piece of *Corda filum* or of *Zostera marina*, found in the shales of the upper strata of the Kentucky coal measures with remains of fishes and of marine shells. But describing such remains and referring them to marine plants would be only an hazardous hypothesis. The stems and leaves of some plants of the coal of Pomeroy are covered with a brownish, thick epidermis resembling the thick leaves of some *Fucaceæ*. But this kind of skin is mixed with remains of ferns and cannot be referred to *Fucoides* without a better proof than this leathery appearance. Prof. Geinitz does not mention any *Fucoides* in his fossil flora of Saxony.

2. *Fungineæ* (*Mushrooms*).

As far as the evidence of fossil plants can be trusted, it seems to prove that species of the mushroom family were living during the period of the formation of the coal. It is especially some of those small, mostly round species of the *Hypoxyleæ* tribe, that have been found attached to leaves and stems of fossil ferns. One of them is described by Prof. Göppert (Syst., p. 262, t. 36, fig. 4), with the name of *Excipulites Neesii* on the leaves of *Hymenophyllites Lobelii*. The same is described again by Geinitz (*Verst.* p. 3, pl. 23, fig. 13,) on the leaves of *Sphenopteris tridactilites*. In this last author we have also, belonging to the same tribe, a *Depaxites Rabenhorsti* and especially the *Gyromices Ammonis*, Göpp. This last species is the only one that has been found heretofore in our Coal measures of America and that I have been able to examine. It abounds on the leaves, the stems and even the naked substance of the shales overlying the coal (No. 3) at Colchester, Illinois. I have seen it also on a piece of fossil stem from Carbondale, Penn. (Specimen No. 711 of Amherst College). It was first named by Göppert, then described and figured by Germar (*Verst.* fasc. 8, p. 112, tab. 39, fig. 169) and more recently by Geinitz. It is a small spiral body about the tenth of an inch in diameter, with the spires progressively enlarging and greatly resembling our *Planorbis parvus* (Say) a small shell now living in fresh water under the leaves and on the stems of floating plants. Its outer end, which in the figures of European authors is blunt and obtuse, appears, on our specimens, abruptly cut and hollow, like the mouth of a thick shelled mollusc. It is finely striated across; and under a strong lens looks like a beautiful small *Ammonites.* From numerous cross sections of good specimens of this small body, I must admit, contrary to the opinions of the learned European authors who have studied it, that it is a true shell, most probably a species of freshwater mollusc. Internally it is hollow, with a hard, thick, parietal substance, generally of lighter color than the shale. It is true that it is often found within the carbonaceous matter of the leaves and of the stems, and thus would appear to have lived within the substance or under the epidermis of the plants like some *Hypoxyleæ.* But at Colchester, at least, it is still oftener found within the naked substance of the shales. Moreover if it was a shell living on the stems and leaves of the coal, it has been of course imbedded by compression as well in the softened woody and carbonaceous matter of the plants as in the soft clay. Germar has already remarked that he found it on shales, without any apparent connection with vegetable substance. But he supposes the possibility of the destruction of all vegetable substance, except that of this small fungus; a supposition which appears somewhat extraordinary. The presence of fresh water shells in the bogs of the coal can not be doubted after the remarkable discovery by Prof. Dawson of a fresh water *Pupa* in the coal fields of Nova Scotia.* And the scarcity of these molluscans in the coal measures is in accordance with what we see on the peat bogs of our time, where the number of fresh water shells is extremely limited. I can not but say again how difficult and hazardous it is to determine such small bodies attached to petrified stems and leaves, from the impossibility of examining their internal structure and of finding the spores. External and variable forms exactly like small fungi are often mere unorganized bodies, produced by some mechanical or chemical action. All the remains of plants and even the shales overlying the semi-anthracite coal of Trevorton, Penn., are covered with small round vesicles, of different sizes, looking exactly like *Spheriæ*, and are filled with a brown powder resembling spores. Sometimes the coal itself is full of them. They have probably been formed by the ebullition of the whole matter accidentally stopped during a strong gaseous emission.

Another very remarkable fungus, *Polyporites Bowmanni*, is described by Lindley and Hutton in the fossil flora of Great Britain, (vol. i, No. 65). This species, or at least one, agreeing exactly with the figure and description of the English authors, was found in black bituminous shales overlying No. 1B coal at Johnstown, Penn. It is an hemispherical or reniform body, marked with concentrical zones, especially distinct near and along the margin. As Prof. Lindley remarked it: "it is like one of those fungi belonging to the genera *Boletus, Polyporus, Thelephora, Dœdalea,* &c., (our species rather resembles a zoned *Boletus*), which attach themselves to their support by one side, projecting forward from it and increasing by periodical additions to their margin, in consequence of which that part assumes a zoned appearance." The lower part of these Fungi, the *Hymenium*, is a more or less thick compound of vertical, cylindrical or angular pipes or tubes closely united together and containing the seeds. It separates more or less easily from the upper part or the hat of the Fungus. Our American specimen is more perfect or better preserved than the two which have been found in England. Two-thirds of the surface show the disposition of the zones of what I consider to be the lower part of the hat or *Pileus*. The balance of the surface, just in the middle of the specimen, is occupied by what appears to be a piece of the undetached *Hymenium*. This part, which is concentrical and exactly in concordance with the zones, is even evidently zoned though less visibly than the *Pileus*, and formed of square, radiate and concentrical areolæ, running around a central point and from it to the margin. This confirmation can not be compared to that of a scale of fish. It is exactly that of a compressed and petrified *Boletus*. The celebrated English author was only prevented from giving the same decided opinion by the mark, on the surface of specimens, of some lines *which are not in accordance with the radial lines near the margin*. This appearance is probably caused by the superposition and agglutination of some piece of another plant. The American specimen has nothing like it and it removes the only objection made against the admission of this species as a true Fungus or mushroom. The black bituminous laminated shales where it was found contain together with it abundant remains of *Lepidodendron*, especially of its leaves and cones.

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* Proceedings of the Geological Society of London, (Dec. 14, 1859).

2. *Lichens, Mosses and Liverworts.*

Brongniart and other palæontologists have already remarked as a peculiar phenomenon, the absence of every trace of fossil Lichens, Mosses and Liverworts in the old formation of our earth. A few mosses and Hepaticæ only have been observed in formations no older than the tertiary,* and especially in pieces of amber in Germany.†

Many years ago, I found around Pottsville, Penn., a kind of laminated soft gray shales, splitting in thin lamellæ and resembling pasteboard, whose surface bears the traces of the remains of very small, innumerable fragments of thin filaments, mixed with an indistinct compound of what appears to be very small detached oval leaflets. At first, I compared them to, and found them to resemble those indistinct forms of leaves and stems, which are seen on the surface of the pieces of peat, when this matter has been formed by the Sphagnaceæ, a tribe of the Mosses. I have come again and again to the examination of those peculiar shales, expecting to find some distinct outline of the plants which cover them with their remains. But nothing more can be seen than what I had discovered at first, and this is not enough to authorize the conclusion that these remains are those of certain mosses.

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* Heer's Flora tertiary Helvetica and also Ungers' Genera; Dunker's Monegr.; and Brongniart's fossil flora.

† H. B. Göppert and G. C. Berendt, die in Bernstein befindlichen organischen Reste der Vorwelt.—Göppert in Berichte der Berlinen Academy, 1853.—Menge Beitraege zur Benstein flora in den Schriften der Nat. Gesell., zu Danzig. Bd. 6, H. 1.

4. *Filices*, (*Ferns*).

The following remark of Mr. Brongniart in his *Tableau des genres*, (p. 15) is fully confirmed by the examination of all the specimens of fossil-ferns that I have been able to collect from our coal measures. He says: "*that he is satisfied that in the classification of fossil ferns, we must establish genera from the attentive study of the nervation and of its relation with the general form of the pinnules and of the fronds. That we can not look for reliable characters to the fructification, until it has been possible to observe the sporanges or fruit-dots of the great majority of the species*," &c. The number of fruiting species of fossil ferns, found in the coal measures of the United States, is already great indeed. But I have not been able to observe in a single case anything else about the fructification, except the position of the spores relatively to the nerves. And this, even, seldom. The true characters used for the classification of the living ferns, viz., the form of the fruit-dots, their mode of attachment and their involucre or *Indusium* could not be ascertained in a single case. Among the most remarkable and distinct fruiting specimens of ferns, I have got two at least, and perhaps three, which show the sporanges separated from the leaves, appearing to be born on separate pedicels, as it happens in many species of our time. One is *Staphylopteris stellata* Lsqx.,* from the sub-conglomerate coal of Arkansas. Another, undescribed represents small bunches of round, flattened sporanges, somewhat inflated on the margins, and attached on a bipinnately divided branch, on both sides of a common narrow rachis, resembling the medial nerve of a *Pecopteris*. The whole bears some resemblance to a fruiting branch of our common *Botrychium Virginicum*. It might be supposed from the disposition of the sporanges, which is like that of *Asterocarpus Sternbergii*, Göpp., that the substance of the leaflets supporting them has been destroyed by maceration; but there is no trace of any such substance and the sporanges are at some place irregularly distributed. A third species, also undescribed, represents a few narrow leaflets, the upper part of a pinna, with very strong, arched, dichotomous, reticulated nerves, marked, in relief on the stone, appearing like the supports of sporanges destroyed by maceration. I consider it as the remains of the fruiting branch of a *Neuropteris*. But all these fruiting branches can not be referred except by mere supposition to species of fossil ferns known by their leaves and classed by their nervation. They must be described separately as fruit and their relation to peculiar species of ferns can not be mentioned, till they are found in connection with their leaves.

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* Second vol. of the Geol. report of the State Survey of Arkansas.

Of course this can not change the views expressed above, concerning the classification of the fossil ferns, but only force the admission of one or more of those artificial and unreliable genera, which may be eliminated by the discovery of better specimens and with which fossil botany has to be satisfied for the present.

With the exception of a few species which can be separately classed as species of doubtful affinity, all our fossil ferns may be contained in the three following tribes: 1st, *Neuropterideæ*, 2d, *Pecopterideæ*, 3d, *Sphenopterideæ*. The great Palæontologist Göppert, in reviewing in his *Genera* (liv. 3 and 4, p. 48), a former classification of his *Systema*, has admitted two other tribes, the *Danaeaceæ* and the *Gleicheniaceæ*. But we have not thus far in our coal measures any representative of the first; and our species of the second, like *Gleichenites artemiscæfolius* Göpp., (*Sphenopteris* Brgt.,) belong by their nervation to the *Sphenopterideæ*. Both these divisions may thus be left aside for the present.

1st Tribe. *Neuropterideæ*.

The classification of our American species seems to necessitate a slight modification in the subdivision of this tribe admitted by the European author. I would subdivide it in the following genera: 1st, *Noeggerathia* Sternb., 2d, *Cyclopteris* Brgt., 3d, *Neuropteris* Brgt., 4th, *Odontopteris* Brgt., and 5th, *Dicthyopteris* Gutb.

1st, *Noeggerathia* Sternb. This genus was first established by Sternberg, *Vers.* p. 28 for the description of the figure of its plate 20th; then by a more appropriate description page 33, and definitively page 36th of his 4th book where he limits as follows the genus *of uncertain affinity: Caudex ignotus; rami teretes, pennam anserinam æquantes, lignescentes; folia alterna, approximata, obovata, ramum basi semi-amplexantia, apice pectinato-dentata, integerrima.* The author does not mention the nervation, but his description is completed and somewhat modified by Mr. Göppert who in his Genera (liv. 5 and 6, p. 107), has described another specimen of Sternberg's species, *Noeggerathia foliosa:* and fixed the genus thus: *Frondes petiolatæ, pinnatæ; pinnæ obovato-cuneiformes vel obovatæ, lateribus petioli applicitis semi-amplexicaules, nervis numerosis teneribus, plerumque simplicibus ab ima basi adscendentibus percursæ.** Göppert then describes two new species of this genus: *N. obliqua* and *N. Beinertiana.* The figure of both represent only part of much longer leaves than those of Sternberg's species. These leaves lacerated on one side have an appearance far different from that of the Ferns, to which nevertheless Mr. Göppert refers this genus. Brongniart, in his *Tableau des genres* has described again this *Noeggerathia foliosa* and from the simple pinnate form of the frond, the rigidity of the leaflets and the mode of nervation, he compares it to a palm or rather to the American *Zamiæ.* He has thus made a separate family of the *Noeggerathiæ*, and places it between the *Cicadeæ* and the *Conifereæ.* This Family contains only two Genera: *Noeggerathia* and *Pychnophyllum*,† this last one replacing the *Flabellaria* of Sternberg or rather including only *Flabellaria borassifolia* Sternb. Mr. Geinitz in the *Versteinerungen*, &c., has apparently admitted Brongniart's views. He places the family of the *Noeggerathiæ* with the Dicotyledonous plants having the same two Genera and refers to *Noeggerathia* a number of fossil fruits of the Genus *Rhabdocarpos.* This variety of opinion and uncertainty of the characters of both the genera *Noeggerathia* and *Cordaites*, is perplexing indeed. But I think that the examination of our American specimens, referred to these Genera may help to fix their true characters and place.

It is evident that both the species published in the report of the State Geological Survey of Pennsylvania as *Noeggerathia minor* and *N. obtusa*, are true *Noeggerathia*, according to the description and the figure of Sternberg's species. They correspond not only with Sternberg's and Göppert's description of the genus, but with the remark of Brongniart about the nervation and the rigidity of the leaves. The branch figured on plate 1 fig. 10, shows that these plants were at least bipinnately divided. Since the publication of this report I have had opportunities to examine better specimens of the same species and especially have lately received from Prof. J. D. Dana of New Haven, the figure of a splendid specimen of *Noeggerathia obtusa*, found in the old Red Sandstone of Montrose, Penn., by Rev.

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* The restriction *plerumque simplicibus* can not be admitted for such leaves as those of *Noeggerathia foliosa* when the nerves all come from the base. They must of course much divide in ascending.

† This genus is wrongly ascribed to Göppert by Geinitz in his *Versteinerungen*, &c., p. 40. In the *Tableau des genres*, by Mr. Brongniart (published in 1849), the author establishes the genus *Pychnophyllum* for replacing *Flabellaria*. Mr. Göppert admitted it only in 1852. Brongniart's genus has even the priority over *Cordaites* proposed by Unger in 1850. But this last name should be preserved as a well merited compliment to Corda who has so admirably described and figured the ***Flabellaria borassifolia*** (Sternb.).

Henry A. Riley. It shows the upper part of a frond with three oblique pinnæ somewhat reflexed from their base and the pinnules or leaves, broadly oval or reniform, the upper one flabellate, all narrowed to the base and pinnately attached on both sides of the rachis by a narrow decurring base. The point of attachment of the leaves is just as I have figured it in my report. This splendid specimen has evidently the general outline and the appearance of a fern and at once puts aside Brongniart's surmise that the simply pinnate form of the leaf, &c., show it to be analogous to the *Zamiæ.*

On the other side, I have been able to find in the Anthracite basin of Pottsville, a stem of *Cordaites* with some of the leaves attached to it. It agrees exactly with the one figured by Corda though less well preserved and entire. It is a simple stem, about half an inch thick, bearing numerous long, ribbon-like, clasping leaves, spirally placed around it, marked with sharp, narrow, parallel, mostly simple nerves. The leaves are scarcely narrowed at the base and thus nearly linear; but, as I have seen it many times and as Corda's figure shows it, the leaves near the top of the stem become shorter somewhat narrowed at the base, spathulate-oblong, just of the same form as both the leaves of *Noeggerathia obliqua* and *Noeggerathia Beinertiana* figured in Göppert's Genera. I will not say that Göppert's species are not true species or belong to the same *Cordaites* as ours; but I believe that all those broken leaves described by Göppert as *Noeggerathia* ought to be referred to the genus *Cordaites* and removed from the Fern-family. I think that the genus *Noeggerathia* should be characterized as follows: *Frond bipinnately divided. Pinnæ, long, linear, oblique, flexuous; pinnules alternately and pinnately placed on both sides of the rachis, enlarged above, obovate, obcordate or reniform triangular, narrowed at the base and obliquely attached to the rachis by a narrow sometimes slightly decurrent base. Nerves equal, numerous, emerging from the base and forking in ascending.* To this genus thus limited I would refer *Noeggerathia foliosa* Sternb., *N. minor* Lsqx., *N. obtusa* Lsqx., *N. flabellata?* Ll. and Hutt., *N. Bockschiana* Lsqx., *Cyclopteris dissecta* Göpp., *C. hybernica*, *C. McCoyana?* *C. Jacksoni* Daws., with some other *Cyclopteris* with a narrow angular base and even perhaps *Odontopteris imbricata* Göpp. If we except of these species *Noeggerathia flabellata* and *N. foliosa* both of the lower coal, the first of England, the second of Bohemia, all the other species pertain to the Old Red Sandstone and thus the botanical classification very well agrees with the geological distribution. For this reason, especially, I doubt if *Noeggerathia cuneifolia* Brgt., of the Permian of Russia belongs to this genus, or it may be, as I have remarked it formerly, that, as this species is found with a *Lepidodendron*, its geological horizon has not been exactly marked.

To the genus *Cordaites* thus characterized: *stem simple, annulate or marked by the persistent base of the leaves; leaves simple, clasping at the base, long-linear, marked by simple, equal, parallel, rarely forking nerves:*—I would refer all those ribbon-like leaves so abundant in our coal measures, and generally found in broken fragments. *Noeggerathia palmæformis* Gopp., recently found in the coal fields of Illinois, *N. Beinertiana* Gopp., *N. ovata*, *N. abscissa*, *N. dichotoma*, *N. tenuistriata*, *N. Bruckeriana*, *N. crassa*, all species of the same author are referable to *Cordaites*. At least as much as can be seen from the figures that mostly represent fragments of leaves.*

2. *Cyclopteris* Brgt. By somewhat extending the definition of the genus *Cyclopteris*, it would be easy to place in it most of the species, if not all, of the genus *Noeggerathia*. But the name of this last genus has the priority to that of the former, and moreover the fine descriptive name, *Cyclopteris*, represents a kind of leaves far different from those to which Sternberg applied the name of *Noeggerathia*.† At first, it contained a number of species established for isolated round leaflets which a more careful observation showed to belong to species of *Neuropteris*. As the relation of species of *Cyclopteris* to species of *Neuropteris* is not easily ascertained, Brongniart made for these doubtful forms a new genus with the name of *Nephropteris*. Numerous American specimens have furnished satisfactory evidence of the identity of most of the species of *Nephropteris* with some species of *Neuropteris* and *Odontopteris*. They are mostly large, round leaflets, placed at the base of the primary pinnæ at the point of union with the common rachis or sometimes pinnules or leaves of an abnormal form, placed on the common rachis between the primary pinnæ or even on the pinnæ, between leaves of a normal form. We can thus eliminate the genus *Nephropteris* and refer to *Neuropteris* the species formerly belonging to it. I can scarcely refer any American species to the genus *Cyclopteris*, as it has been limited by Brongniart in his *Tableau des Genres: frond simple, pedicellate, symmetrical, round, cordiform* or *flabellate, entire* or *lobed, without traces of medial nerve, all the nerves coming out from the basis of the leaf and diverging in dividing or forking to the border.* I have found only a small incomplete specimen of *Cyclopteris flabellata* Brgt. It is no longer in my possession and I am unable to say if it represents exactly the European species. I know nothing more of it but what is said on p. 855 of the Report of the Pennsylvania survey.

3. *Neuropteris* Brgt. The celebrated Göppert had in his *Systema* admitted a new genus, *Adiantites* for a number of these

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* Prof J. W. Dawson has adopted the same views in a paper recently published in the Canadian Naturalist.

† Jac. Nöggerath to whom the genus is dedicated is author of valuable researches on fossil trees and stems published at Bonn, 1820–21.

species intermediate between *Cyclopteris* and *Neuropteris.* But in his *Genera*, liv. 5 and 6, p. 90, he puts the classification aside and admits Brongniart's genus *Cyclopteris* with some modification, so as to place in it a number of species which I consider true species of *Neuropteris.* Indeed, the essential character separating his genus *Cyclopteris* from *Neuropteris* is, for the former, the nerves emerging from the base of the leaves and flabellately diverging and dichotomous in ascending; while in the genus *Neuropteris*, the medial nerve is marked from base up to above the middle of the leaves and disappears near the summit. Now, it is certain that a number of our *Neuropteris* have for the same species the two kinds of nervation that characterizes Göppert's genera. In *Neuropteris hirsuta* Lsqx., the long leaves are generally strongly nerved up to the middle; while the round leaflets, attached at their base, as also the upper leaves of the penæ, which become simple, have all their nerves flabellate from the base without a trace of a middle nerve. *Neuropteris Clarksoni* Lsqx., bears on the same pinnæ two kinds of leaves, the one longer, narrower and strongly nerved in the middle; the others shorter, broader, with all the nerve flabellate from the base. This *Neuropteris* appears to be the American representative of *Neuropteris auriculata* of Europe, essentially differing from it by the strong medial nerve that marks some of its leaves. In *Neuropteris Loschii* Brgt. so common in our coal-fields, the medial nerve appears only with the larger leaves of the inferior secondary pinnæ. In *Neuropteris Desorii* Lsqx., again, the leaves attached to the primary pinnæ are marked with a strong medial nerve, ascending nearly to the point, while a number of leaves, attached on the primary rachis, have not only all their nerves flabellate from the base, but are enlarged in outline and take the form of *Cyclopteris.* All our species of *Neuropteris* show the same variety of nervation. Accordingly, I think that it would be more convenient and rational at the same time, to fix the genus *Neuropteris* as follows: *Frond pinnately, bi- or tri-pinnately divided; pinnules or leaves of various forms, round, oblong, oval, mostly entire, sometimes lobed, cut or fringed, attached to the rachis only by the middle point of their base. Medial nerve sometimes distinct and vanishing above; secondary nerves numerous, either obliquely emerging from the medial nerve or flabellate from the base, all arched and dichotomous.*[*]

4. *Odontopteris.* The only essential character which separates this genus from the former is that in *Odontopteris* the pinnules or leaves though separated from each other, are attached to the rachis by their whole base or by the greater part of it. The nervation is very variable; the nerves either running from the whole base and straight as in *Odontopteris Schlotheimii*, or emerging

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[*] As the genus is here limited, both the species *Neuropteris Moorii* Lsqx., and *Neuropteris Adiantites* Lsqx., ought to be separated from it and placed elsewhere.

from an enlarged base, flabellate, dichotomous, and arched as in the larger leaves of *Odontopteris Alpina,* Sternb. This last species, a beautiful one, has been lately found in the Anthracite coal measures of Rhode Island,* together with a number of our common coal plants, mostly the species characteristic of No. 3d and No. 4th coal. Our specimens are far more complete than any of those published in Europe. The frond, tripinnately divided has the primary pinnæ very obliquely separating from the common broad, narrowly and equally striated rachis. The secondary pinnæ are alternate and open or perpendicular to the rachis. The leaves or pinnules of the lower pinnæ are distinct, somewhat distant, oblong, obtuse, slightly scythe-shaped outward, one to two inches long, attached to the rachis by two-thirds of the rounded base. In the upper part of the frond, the secondary pinnæ are shorter and their leaves or pinnules are also much shorter, reniform or ovate, sometimes united together by the decurrent base, the terminal pinnule is very long, oval-lanceolate, obtuse. Generally the pinnules are shorter on the upper side of the pinnæ, a character already slightly marked in Sternberg's figure. The leaves of the lower pinnæ have exactly the nervation of the genus *Cyclopteris* of Göppert or of the *Neuropteris* with flabellate nervation. On the upper pinnæ, the leaflets even in the same pinna have either a flabellate nervation from the middle of the base, or more generally the nerves emerge from the rachis along the whole base of the leaflets, thus showing that the species is a true *Odontopteris,* as Geinitz has remarked it.

Large specimens of *Odontopteris Schlotheimii* Brgt., obtained from our coal-measures, show that this species found only in fragments in Europe, is tripinnately divided. The primary pinnæ about one foot long are lanceolate, half open; the secondary ones are linear, pinnately lobed with the lobes round or oval, united together near the base or distinct. In this species as in the upper pinnæ of *Odontopteris Alpina,* the first leaflets near the point of attachment of the rachis are generally distinct and their nerves run out from a point in the middle of the base of the leaflet and not along the whole base as in the other divisions.

Another fine and remarkable species of *Odontopteris* was also found recently in connection with No. 1B at Murphysboro, Ill. It is *Odontopteris heterophylla* Lsqx., (in MSS. report of the Geol. State survey of Illinois, pl. 2, fig. 2 to 5). The frond appears bipinnately divided. Pinnæ lanceolate in outline or cordate-ovate. Pinnules or leaves, either entire, obovate-obtuse decurrent on the rachis, becoming broader, shorter, cuneiform-oval near the base of the pinnæ; or elongated, diversely lobed, with unequal, linear-long, or short-lanceolate-oval divisions; terminal pinnule large, deltoid, obtuse, alternately lobed on each side. Nerves sharply marked, inflated near the base two or three times forked; in the decurrent leaflets, the nerves are also a little decurrent along the rachis; in the other leaflets they run out from the whole base.

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* I am indebted for the communication of a fine specimen of it to Mr. James H. Clark of Newport, R. I.

5. *Dictyopteris* Gutb.—This genus so well characterized by its nervation has still but one representative species, abundantly found in the whole extent of our coalfields, *Dictyopteris obliqua* Bunb. The form of the leaves is variable like their size. The upper pennæ are only pinnately lobed and the lobes separated to the middle, and short and nearly round.

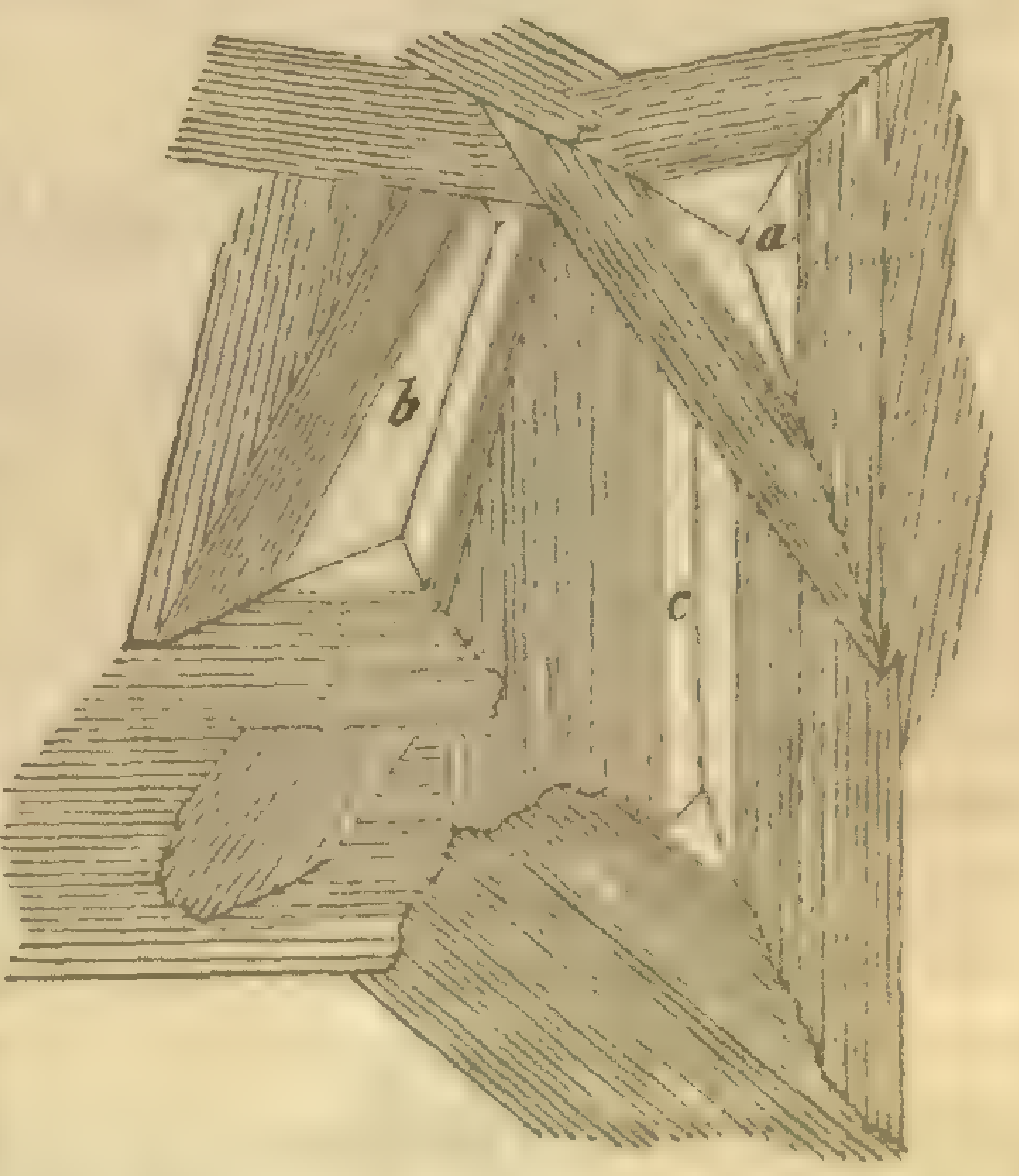
Columbus, Ohio, July 4, 1861.

(To be continued.)

Art. XXIV.—*Observations upon the Freezing of Water at the Passaic Chemical Works, Newark, N. J.;* by Artemas Bigelow.

One of the furnaces having been stopped in the early part of the month of Jan., 1860, the cistern for cooling acid before it is bottled, became frozen on its surface about two inches deep. The cistern was within the building and near the side of it. The weather but moderately cold. On the side farthest from the building the water had oozed up through and overspread the surface, slowly freezing at the same time. Attracted by the appearance of this portion of the ice, I examined, and found it composed of plates or laminæ lying one against the other obliquely to the surface of the water. The masses of plates were also at various angles to each other, leaving many open spaces of regular forms, *a*,*b*,*c*, fig. 1, whose faces were beautifully smooth and perfect, meeting below from their obliquity. The origin of each mass of plates being independent of the others, and extending from below obliquely upwards in three or four different directions, a cavity would consequently be left between the divergent masses, (fig. 1). These laminæ were free, except below, so that a knife thrust underneath lifted them out separately. Also as often as the thumb was applied with pressure to their upper edges, the lines would disappear and the ice at the place of pressure look solid,

1.



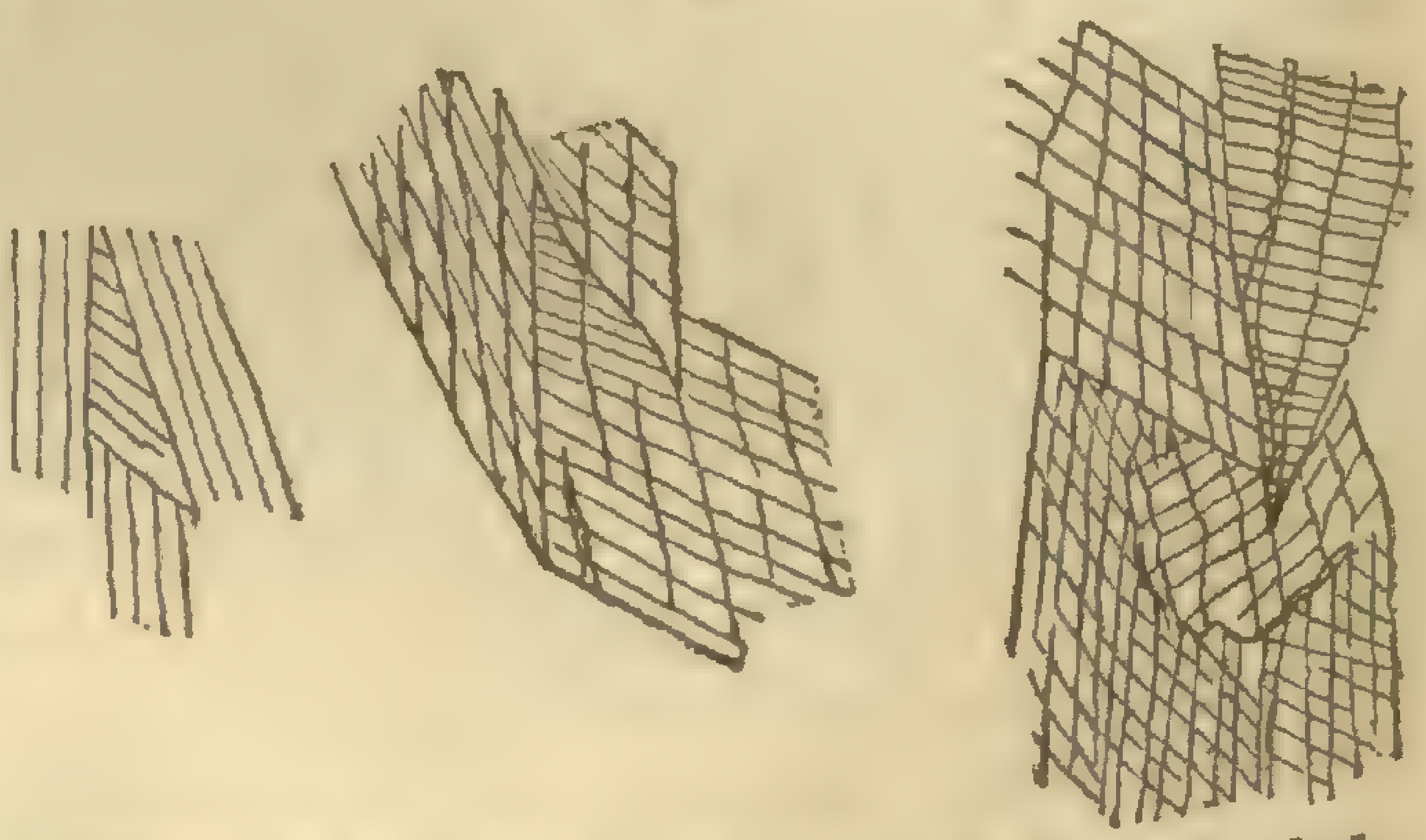
a, *b*, *c*, are cavities, the inclined sides meeting at the bottom.

while moisture oozed out on the surface; but, on the instant of its removal the plates regained their appearance and position; showing that there were intervening spaces filled with water. The extent of surface was about two square feet.

Directing the water to be drawn off, I left it till the next morning, when, the cold having increased, the ice was found solid and transparent, the lines obliterated and the cavities remaining. The next day was warmer, the ice began to melt, and the plates were again apparent, less perfect than before but defined and so loose as to crush under the least pressure. Wherever it was still firm, pieces often a square inch in extent, broken off, frequently showed a brilliant iridescence. The iridescence was in the structure and appeared to be from the surface of a plate not in the same plane with the series adjoining it. When seen edgewise it presented a well defined line like a fine crack, but no separation could be made by a knife any more in that than in any other direction.

A few days after, I found the water in the urn on the top of the office stove frozen except a central cavity. A fire had been made and the ice began to melt. On the top, at the centre, the plates were visible, but imperfect from the melting. Taking out the mass and holding it up to the light, the inner surface was beautifully marked throughout with strong lines at similar angles to each other as existed in the plates above first mentioned, but crossed by finer lines invariably at an acute angle, forming rhombs whose angles were slightly rounded. Fig. 2.

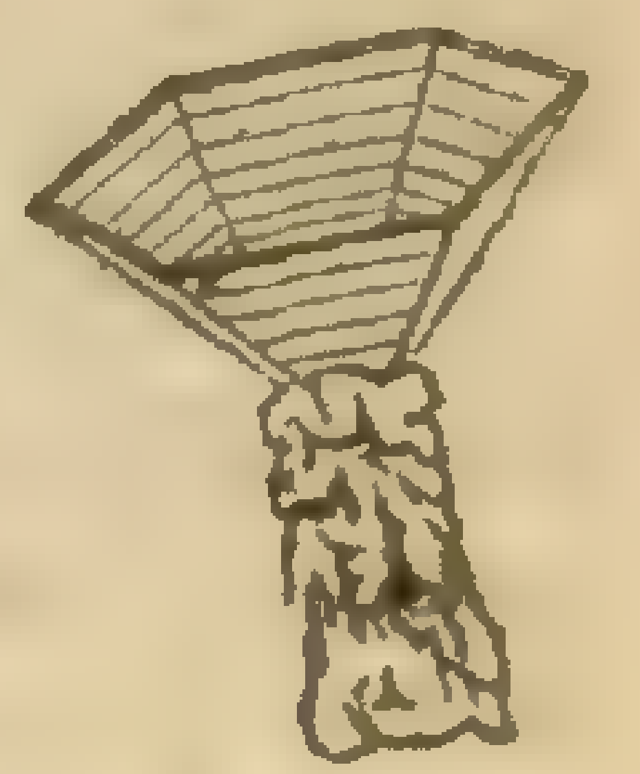
2.



I looked daily for these appearances, as I passed back and forth, for nearly a mile, between the canal and the river, and frequently during the mild weather of the succeeding winter months observed the facts mentioned in the first case, with this difference, that when the cavities were apparent in thin ice formed as usual on the surface of the water, the sides diverged downward, being the reverse, in position, of those mentioned in case first. When water freezes rapidly, the originating centres must be many, the series of plates of small extent, the cavities filled up, a homogeneous mass formed, and no sign of its structure apparent until the slow thawing of spring reveals it, as we shall soon show.

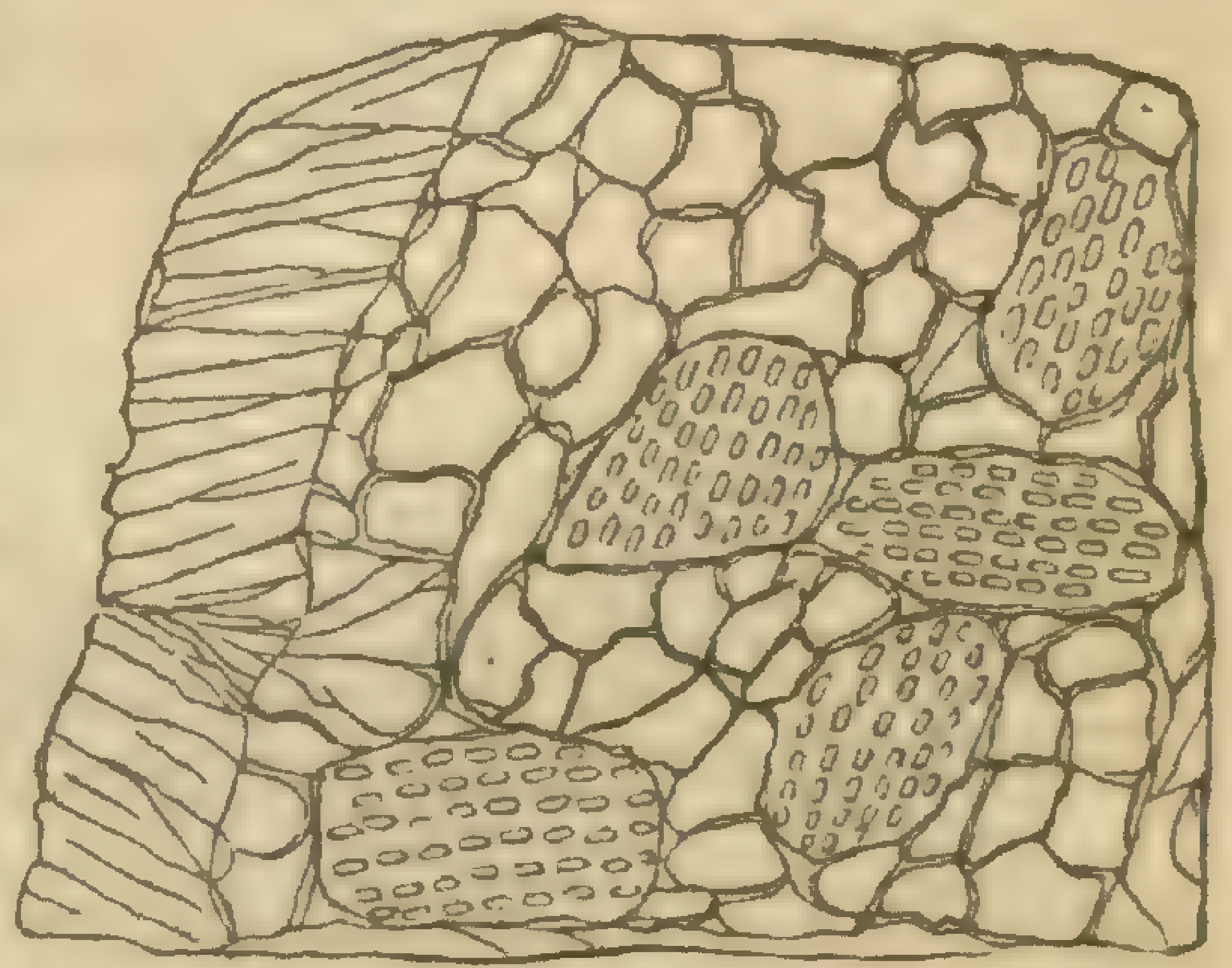
Again, one morning in February, stepping down to the edge of the river I noticed glistening particles of ice upon chips and sticks in great abundance, which proved, on examination with a magnifier, to be hexagonal hopper-formed bodies upon small icy pedestals. The diameter across the top of the hexagon was about one line. The form was very perfect and beautiful, with fine lines running around as if they were the divisions of the crystalline layers, as is shown in the accompanying magnified drawing. Fig. 3.

3.



During the month of February, masses of ice were piled up along the shore above the reach of the tide, and after exposure to the warm sun of that month, lost their vitreous transparency and became nearly snowy white. Their surface was divided up into irregular or oval forms separated by grooves, and lines ran through their thickness, like the joinings of a compact mass of irregular crystals. When a large mass was thrown or let fell upon another mass of ice it broke easily into innumerable crystals. These crystals are irregular and can be called such only to distinguish their general appearance, but almost uniformly show the obliquity mentioned above.

4.



One such mass, fig. 4, had in the large divisions on its surface, oval spots, separated by depressions due to more rapid melting in the joinings. They struck me as parallel to the second case mentioned above, except in this case the angles are completely rounded off by melting.

The noticeable points in these facts seem to be, 1. That water in freezing, under favorable conditions of temperature, forms plates containing between them thin layers of water. Why these water layers do not freeze at such temperature I am unable to tell, unless the air bubbles of the parts crystallized are driven into the intervening spaces and there confined, preventing the water layers from losing heat so rapidly. Whatever be the reason of its higher temperature, the same cause enables those layers when frozen, to melt more easily than the plates, even to the depth of many inches. If the idea be correct that the water layers contain numerous minute air bubbles, then the sun light in passing through the ice, must give up accumulating heat to these bubbles, like the sun shining into an air-tight glass globe. But whatever the cause, this method of freezing and thawing accounts for the manner in which the ice of the great lakes and other large bodies of water breaks up in the spring. The presumption is that all ice forming on water is produced in the same manner. But at a very low temperature, the crystallizing centres are so many and the freezing of the plates and intervening spaces so rapid, that the process could not be distinguished. Yet all field ice from this latitude to the arctic regions breaks up in the same manner, by first becoming porous or "rotten." I would ask, can the snows of the glaciers when turning to ice, go through such a process of crystallizing, and can it have anything to do with its supposed plasticity?

2d. The uniform obliquity of its plates, the rhombic markings in case 2d, and the oval spots in the last case indicate a rhombohedral form in the ultimate crystals.

Whether observations similar to these have been made by others I do not know, but they interested me and led me to make the accompanying drawings, and also stereoscopic pictures of some ice masses which had lain exposed for many days to the action of warm sun light. I had intended to renew these observations last winter, but circumstances prevented me from doing so.

ART. XXV.—*Note on the probable Age of the White Limestone, at Sussex and Franklin Zinc Mines, New Jersey.* In a letter to Profs. J. D. DANA and BENJ. SILLIMAN, Jr.; by GEORGE H. COOKE, Prof. of Chemistry, &c., New Brunswick, N. J.

Dear Sirs,—At our late visit to Franklin Furnace, Sussex Co., N. J., I had the pleasure of pointing out to you, the faithfulness of Vanuxem and Keating's original description of the geological occurrence of the rocks of that locality; and the correctness of their conclusion, that the white and blue limestones found there, are of different ages. Since their description was written it has been asserted that the white limestone is of the same age as the blue limestone, though changed in appearance by metamorphic action.

Being confident that the facts cited prove the correctness of the former conclusion and the consequent incorrectness of the latter, I quote portions of the paper which bear upon the above points.

The original article is published in the Journal of the Academy of Natural Sciences, Philadelphia, vol. ii, p. 277, and is entitled, "On the Geology and Mineralogy of Franklin, in Sussex Co., N. J., by L. Vanuxem and Wm. H. Keating." They say, "The whole country to a great distance is composed of what may be considered as sienite. . . . The sienite of Franklin is found in beds or layers of variable thickness, running in a direction parallel to that of the ridge from the N.E. to the S.W. . . . The layers or beds incline to the S.E., dipping under at an angle of about 80°. Subordinate to this sienite are found limestone, gneiss, and greenstone. . . . The limestone forms a bed without any apparent parallel seams or divisions, and is peculiarly characterized by its eminently crystallized structure, consisting of large straight lamellar masses, confusedly aggregated. . . . It is of a fine white color, presenting in some instances, a pearly lustre, slightly chatoyant. . . . The direction, inclination and dip of this limestone are the same as those of the aforementioned sienite.

"Next to the sienite, but evidently of a later formation, is found a mass of grauwacke of no great thickness. This grauwacke is generally fine grained, of a light gray color; the fragments, as well as the cement which connects them, appear to belong to quartz. It is found on the north side of the ridge in thin beds or layers, directed from the N.E. to the S.W. and dipping to the N.W.; this grauwacke is evidently of posterior formation to the sienite, and must have been formed after the surface of this rock had undergone those changes which we at present observe in it; for instead of presenting a parallel stratification, it is found inclining in a diametrically opposite direction, and covering the edges or crest of the layers of sienite. The grauwacke and its mode of superposition can be well observed on the road from Franklin to Dr. Fowler's (house) at about a quarter of a mile below the furnace; it is covered by a blue limestone which rests upon it in parallel superposition; this limestone is found in layers or beds of variable thickness; its color is a pale gray sometimes of a deeper gray passing into blue; its texture is compact or subsaccharoidal; near the grauwacke it is slaty."

A locality, where the meeting of the two rocks is plainly exposed, is on the left of the road passing from the tavern at Franklin Furnace to Col. Fowler's and but a few rods from the former place. The gneiss will be seen dipping at an angle of 75° to the S.E. and the sandstone (grauwacke) which has the lithological appearance of Potsdam sandstone, lying immediately on the upturned edges of the gneiss and dipping at an angle of 45° to the N.W. The strike of the two rocks is different;—the sandstone being 15° or 20° more to the S.E. and N.E. than the gneiss. The sandstone is as near as I can judge from its limited exposure not more than 30 feet thick; the slaty limestone, which lies upon the sandstone, is somewhat thicker; perhaps 70 or 80 feet thick, and then comes a cherty limerock of great thickness. No fossils have been found in this vicinity, either in the limestone or sandstone. Dr. Kitchell, Superintendent of the late Geol. Survey of New Jersey, recognized the unconformability of the gneiss and the blue limestone and in his Annual Report for 1855, p. 133 and onwards, refers to several localities where this can be seen.

New Brunswick, N. J., July 2, 1861.

Art. XXVI—*Chemical Contributions*; by M. C. Lea.

4. *On the Action of Nitric Acid on Picramic Acid.*

On this point very conflicting statements have been made. Girard, and Pugh, respectively state that picric acid is reproduced by the oxydation of picramic acid by nitric acid. A similar statement is made by Kolbe in his Lehrb. d. Org. Chemie (authority not given). In a paper published several years since, on picric acid, I expressed a similar opinion. On the other hand Wöhler stated that his nitrohæmatic acid (now known to be identical with picramic) was not reconverted to picric acid by the agency of nitric acid. Gerhardt too, in quoting the first opinion puts a note of interrogation after it, as if to express a contrary conviction. These differences of opinion have induced me recently to re-examine the subject and have led to the conclusion that the substance formed is not identical with picric acid. The following were the reactions observed.

Picramic acid readily dissolves in strong nitric acid to a dark brown solution. By 15 minutes boiling this becomes clear bright red. If then saturated with potash, quantities of nitrate of potash crystallize out, with much brown varnish, but no trace of picrate. After one hour's boiling the color of the solution is considerably lighter—the results much the same.

After four hours boiling the color of the liquid was bright yellow. It was evaporated in the water bath and gave a crystalline substance mixed with much resinous matter. To remove this, it was dissolved in as small a quantity of cold water as possible, filtered and mixed with half its bulk of strong sulphuric acid. On cooling, a crystalline reddish yellow substance separated which might easily be taken for picric acid mixed with resinous impurity. But neutralized by ammonia and heated with sulphydrate of ammonia it gave no indications of the presence of picric acid. Tested with cyanid of potassium the results were the same. By spontaneous evaporation of the solution of the substance in ammonia, fan-shaped groups of hair brown needles were obtained. Analysis of these showed conclusively that they consisted of *oxalate of ammonia* disguised by organic matter.

After eight hours boiling the liquid was pale straw yellow and by evaporation on the water bath yielded a substance dissimilar from the former, bright yellow, and colored intensely deep red by cyanid of potassium after previous supersaturation with ammonia. But treated with sulphydrate of ammonia, it gave no indications of the production of blood red picramate. but became greenish brown with production of a greenish precipitate. The presence of oxalic acid could not be detected.

These experiments appear to me to leave no doubt that picric acid is not formed by the action, either brief, or prolonged, of nitric acid on picramic acid, but that resinous substances are produced, accompanied after a time by oxalic acid, which at a later stage, suffers decomposition itself. All of these substances are however produced in very small amount, the greater part of the constituents of the picric acid passing off in volatile decomposition products.

5. *On the Preparation of Picramic Acid.*

We are generally directed to dissolve picrate of ammonia in alcohol, saturate with ammonia, and then with sulphydric acid. These saturations are tedious and troublesome, and as picrate of ammonia is but sparingly soluble in alcohol much of the latter is consumed, and the solutions are very bulky. The following process will be found greatly preferable.

Picric acid (which is very soluble in strong alcohol) is dissolved in cold alcohol, and excess of sulphydate of ammonia added. The liquid then only requires to be evaporated over the water bath, the residue to be exhausted with boiling water, filtered, and treated with acetic acid. The picramic acid obtained in this way is very pure, and the quantity large. In one experiment where the quantities were weighed, over 63 per cent of the weight of the picric acid consumed was obtained. If too little sulphydrate be used, picric acid remains in the mother water from which the picramic acid crystallizes, and may be recovered by precipitating with carbonate of potash.

Philadelphia, July 13, 1861.

Art. XXVII.—*On the Production of new Coloring Matters by Decomposition of Nitronapthaline and Dinitronaphthaline;* by M. Carey Lea, Philadelphia.

(1.)

In the process for preparing naphthylamine by the action of acetic acid and iron on nitronaphthaline, the nitronaphthaline is placed in a retort with iron filings and acetic acid, and after the first action has passed off, the contents are heated, a receiver attached and hot lye is added to disengage the naphthylamine. But if a well cooled receiver be attached at the outset of the operation and if heat be applied for some time before the addition of the caustic alkali, a liquid passes over which exhibits the following reactions.

It has a pale reddish color and exhales the disgusting odor of naphthylamine. The pale reddish color becomes pale violet by addition of mineral acids. If it is placed in an open capsule and heated on the sand bath with addition of dilute sulphuric acid, the pale violet color gradually deepens in intensity to rich blue purple. After a time, a black crystalline precipitate falls which must be separated. The brown filtrate by further heating, again becomes rich purple and deposits a further quantity of precipitate. But eventually the liquid becomes muddy brown (A) and yields no more of the precipitate.

This latter is produced at best in extremely small quantity, and sometimes scarcely appears at all—a grain or two is all that can be obtained from 50 or more gramms of nitronaphthaline.

The properties of this very interesting and beautiful substance, as far as could be determined from the very scanty amount obtained for examination* were as follows. As caught on the filter, it constituted nearly black needles with a most brilliant golden green glitter. After being dissolved in alcohol, it was obtained as a dark red powder, which when placed on glass and a platinum spatula drawn a few times over it, gave a bright green, almost metallic reflecting surface, contrasting strongly with the red powder around it.

It dissolved somewhat readily in alcohol coloring it an intensely deep blood red. The addition of a very small quantity of sulphuric or nitric acid brought this through a succession of shades as the quantity of acid increased, first ruby, then crimson, then rich purple and finally blue purple, all of the richest shades, and so intense as to require great dilution to render the solution at all transparent. The substance exhibits considerable resistance to acids. The alcoholic solution acidulated with sulphuric acid may be boiled without destroying the color; if nitric acid be substituted, the solution by boiling becomes pale straw color, possibly an effect of the reaction of the nitric acid on the alcohol present.

The production of a red color by alkalies and a blue by acids is becoming characteristic of a large number of organic coloring matters. Amongst these are, the coloring matter obtained by Church and Perkins from tincture of madder; by the resinous body obtained by Schiff in the spontaneous decomposition of naphthylurea; by the body obtained by Church and Perkins from nitrosonaphthaline;† by carotin, as observed by Dr. Husemann;‡ by a blue coloring matter obtained from picric acid described by myself. The frequency of this reaction is constantly increasing as we become better acquainted with organic coloring matters.

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* Much to the author's regret he was obliged to discontinue this examination in consequence of an unexplained injurious effect upon his health by manipulating with naphthylamine.

† Jahresbericht, 1857, p. 390.

‡ Chem. Centralb., May 1861, p. 347.

The substance which I have here described would doubtless be valuable as a dye if it could be obtained in sufficient quantity, for the richness of its colors leaves nothing to be desired, but it is only a secondary product in the reaction which produces it. Until it can be obtained in sufficient quantity to admit of its constitution being determined, I propose to call it Ionnaphthine, from ιον, *a violet.*[*]

(2.)

If the muddy brown liquid mentioned at (A) in the 2d paragraph be treated with liquid ammonia, brown flakes separate. If these be treated with dilute sulphuric acid and bichromate of potash they become black. They then do not dissolve in water or alcohol, but dissolve in dilute nitric acid to a deep violet solution, greatly inferior however in color to the solution of ionnaphthine. This substance may possibly be identical with that described by M. Du Wildes[†] and obtained by him by oxydating napthylamine by means of nitrate of mercury.

(3.)

If the solution of dinitronaphthaline in alcoholic ammonia be heated with solution of sulphite of ammonia, the red solution assumes a rich deep rose color, far richer and more brilliant than the original solution. I have not as yet been able to isolate this substance.

Dinitronaphthaline is as fruitful in colored derivatives as aniline. Treated in solution in alcoholic ammonia with stannous chlorid, it yields a fine blue. Roussin's "artificial alizarine" affords fine shades of purple, the reaction is obtained with great facility. Hofmann and Wood's ninaphthylamine as I have obtained it varies from copper to sealing wax red, but does not seem to me likely to be valuable as a dye. Roussin's alizarine will no doubt be very much so.

ART. XXVIII.—*The Primordial Zone of Texas, with descriptions of New Fossils;* by B. F. SHUMARD.

IN the appendix of Dr. Ferd. Rœmer's volume of Explorations in Texas, published in 1842, we find a notice of Trilobites and other fossils from the valley of San Saba river, which are referred by the author to the epoch of the Lower Silurian system, and subsequently in his *Kreidebildungen von Texas* published in 1852, Prof. Rœmer has given a more extended description of these fossils illustrated with excellent figures. For one of the trilobites Rœmer proposed the genus *Pterocephalia*, which appears to be nearly related, if not identical with *Conocephalites* of Zenker, another and unnamed species possesses all the characters of *Dikelocephalus* of Owen, while a third erroneously figured as the tail of the latter is evidently the head of an *Arionellus*.* It is scarcely necessary to inform palæontologists that these genera are confined exclusively to the Primordial Zone, to which, indeed, Mr. Barrande has already referred the Texan strata, basing his opinion upon the evidence afforded by the work of Rœmer, last cited.

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[*] The specimen of this new coloring matter which Mr. Lea has sent to us sustains all he says of its beauty.—EDS.

[†] Rep. de Chimie appliquée, Mai, 1861, p. 172, M. du Wildes is in error in supposing that a reaction which he has obtained is the first instance of a reproduction of an original body from a nitro-substitution compound.

We have no further account of the Primordial rocks of Texas, until 1859, when the present writer published a notice of their discovery in Burnet county (Trans. Acad. Sci., St. Louis, vol. i, p. 673,) in which their parallelism with the Potsdam sandstone and Calciferous sand group of Iowa, Wisconsin and Minnesota and the magnesian limestone series (in part) of Missouri was indicated.

Further explorations have shown that the Primordial rocks with their characteristic fauna are spread over considerable areas in the counties of Burnet, San Saba and Llano, and that they also extend into McCulloch, Mason and Lampasas, and as comparatively little is known respecting their lithological and palæontological features in this, their most southern outcrop hitherto recognized on the American continent, I propose in the present paper to give a brief description of them, drawn from a somewhat careful study of their characters at a number of localities visited by the Geological Survey.†

The Primordial Zone of Texas may be described as a series of light colored, pure and impure dolomites, limestones, chert, calcareous and silicious sandstones, gritstones and conglomerate, presenting an aggregate thickness of from eight to ten hundred feet, and separable into two well marked divisions, of which the superior represents the Calciferous sand group and the inferior the Potsdam sandstone of the northwest.

These rocks are based on reddish feldspathic granite, very similar in lithological character and composition to the granite, which occurs in the region of the Iron Mountain of Missouri, and they are succeeded by even-bedded, hard, brittle, remarkably close-textured, pure limestone and alternating beds of very compact dolomite, sometimes elegantly variegated with delicate flesh-colored cloudings. This formation, some of the beds of which resemble lithographic limestone, has received the name of Burnet marble, and may possibly represent the Bird's-eye limestone of the New York series. The fossils hitherto discovered in it are chiefly *Orthoceras* and *Straparollus*, but the few specimens we have found are so badly preserved that they are almost useless for the purpose of identifying the age of the formation.

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* It affords me pleasure to acknowledge here, that in these explorations I have been materially assisted by Prof. W. P. Riddell of the Texas Geological Survey.

† Rœmer's fossils were found in the San Saba valley, McCulloch county.

CALCIFEROUS SAND GROUP.

This formation lies immediately beneath the Burnet marble and consists of calcareo-magnesian limestone, nearly pure limestone and chert. The usual characters of the group may be understood from the following sections:—

Section of Falls of Deer Creek, near Colorado River, Burnet Co.:—

No. 1. Light-colored calcareo-magnesian limestone with much chert, - - - - - - -	40 ft.
" 2. Alternations of light gray, thick-bedded, compact and subgranular, calcareo-magnesian limestone, and nearly pure white, coarsely crystalline limestone, often handsomely variegated with pink and purple. The upper beds are more compact and of finer texture than the lower, - - - -	299 ft.
" 3. Very hard, light gray dolomite with a pink tinge,	57 ft.

At Marble Falls, Colorado river, just below the mouth of Flat-Rock creek, we have an interesting section, showing the connection of the Calciferous sand group with the overlying formations thus:

No. 1. Dark, Carboniferous Limestone with Productus semireticulatus (?) Chonetes, Zaphrentis and numerous remains of Crinoidea, - - - - - - -	45 ft.
" 2. Alternations of nearly pure, brittle limestone and dolomite (Burnet Marble), - - - - -	90 ft.
" 3. Thick beds of very hard, brittle limestone (Burnet Marble),	55 ft.
" 4. Calciferous sand group, consisting of light and dark gray, compact and fine-grained dolomite, in beds from one to six feet thick, - - - - - -	80 ft.

At Mormon Mills, on Hamilton creek, five miles above its confluence with the Colorado (Burnet county) we find the following section, showing the junction of the formation under notice with the Potsdam sandstone:—

No. 1. Massive beds of sub-crystalline, calcareo-magnesian limestone, variegated with brown and purple and forming rough cliffs, - - - - - - -	290 ft.
" 2. Gray micaceous sandstone, made up of fine grains cemented with argillo-calcareous matter, - - - -	5 ft.

Thus characterized the Calciferous sand group of Texas is largely developed in the counties of San Saba, Llano, McCulloch, Menard, Mason and to a limited extent in Lampasas. Its presence may in general be recognized by the rough and picturesque appearance of the country where it prevails.

Along the Colorado for some distance above the mouth of Brady's creek and throughout the whole course of the latter stream in San Saba county and Deer Creek in Burnet county, this formation is characterized by high castellated and mural escarpments, which are strikingly similar in their appearance to the bluffs of 3d or *Lead Bearing Magnesian Limestone* of the Osage, Gasconade, and Niangua in Missouri and those of the same geological age (*Lower Magnesian Limestone*) that escarp the upper Mississippi in Iowa, Wisconsin and Minnesota. In the districts drained by the branches of Morgan's creek in Burnet county, and toward the head of Wallace creek and various other points in the southern part of San Saba, the formation contains much chert, both compact and in the form of large rough cellular masses with cavities coated with brilliant crystals of druzy quartz. The beds are also not unfrequently traversed in various directions by spar veins. In fact they present all the characters of the productive lead-bearing rocks of Missouri and are of the same geological age. It is therefore not at all improbable that valuable deposits of lead will be found within the districts underlaid by these strata in Texas.

Fossils.—In the chert beds of the formation we have found at a few localities *Pleurotomaria*, *Ophileta* and *Orthoceratites* identical with species discovered by Prof. Swallow and the writer in the 3d Magnesian Limestone of Missouri. No fossils have yet been found in the calcareous beds of the formation in Texas.

POTSDAM SANDSTONE.

This important division of the Primordial zone in Texas has a thickness, estimated at not less than five hundred feet. The sections given below taken by the writer in Burnet county, assisted by Dr. W. P. Riddell and S. Heron, Esq., will serve to show the character of the different beds that constitute the mass.

Section at a small creek, five miles N.W. of the town of Burnet:—

No. 1.	Soft, chalky limestone with well marked cretaceous fossils,	50 ft.
" 2.	Gray and bluish gray, hard limestone, of a sandy texture, in beds from a few inches to a foot thick, and containing *Arionellus* (*Bathyurus* (?) *planus*, *Bathyurus depressus*, *Camerella* sp., *Orthis Coloradoensis*, *Orthis* sp.? *Discina microscopica* and *Lingula* sp.? - - - - - - - -	81 ft.
" 3.	Mottled gray, purple and greenish, earthy and sub-crystalline limestone, with alternating bands of silicious limestone—contains the same fossils as No. 2, - - - -	45 ft.
" 4.	Slope with projecting ledges of No. 3, - -	55 ft.
" 5.	Slope, - - - - - - - -	24 ft.
" 6.	Schistose coarse-textured limestone, made up of crystalline particles and abounding with fragments of trilobites, chiefly *Arionellus*, - - - - -	20 ft.

Section taken about one mile south of the preceding at head of Clear creek:—

No. 1.	Cretaceous strata in horizontal beds,	110 ft.
" 2.	Slope, covered with conglomerate, composed of pebbles of sandstone and calciferous sand rock, rather firmly cemented,	12 ft.
" 3.	Potsdam sandstone, consisting of thin layers of variegated green, gray and purplish, sandy limestone, with bands of dolomite and silico-calcareous rock interstratified. Some of the beds are highly charged with trilobites, of which the most common are *Arionellus Texanus*, *Dikelocephalus Rœmeri* and *Conocephalites Billingsi*,	40 ft.
" 4.	Highly ferruginous silicious sandstone, composed of fine grains, loosely cemented, passing downwards into coarse gritstone and conglomerate. The upper beds contain *Lingula* and *Obolus* (?)	60 ft.
" 5.	Flesh-colored granite, interstratified with veins of milky quartz,	6 ft.

Section near mouth of Morgan's creek:—

No. 1.	Coarse-textured, variegated purple, green and brown limestone, soft and crumbling readily to sand,	52 ft.
" 2.	Greenish and gray calcareous sandstone made up of fine grains,	8 ft.
" 3.	Slope, with projecting ledges of white, green and gray limestone with *Conocephalites Billingsi*, *Agnostus Coloradoensis* and *Arionellus*,	27½ ft.
" 4.	Thin beds of calcareo-silicious limestone with same *trilobites* as No. 3,	60 ft.
" 5.	Brown ferruginous sandstone with *Lingula*,	60 ft.
" 6.	Flesh-colored granite,	110 ft.

The best exhibitions of this group of rocks occur in the region of Morgan's creek and its tributaries in Burnet county. The remarkable elevation in this region, known as the "Tatur Hill" is composed of this rock. This hill is of truncated conical form, and rises to the height of two hundred and eighty-four feet above the adjacent valley. The Potsdam sandstone is also finely displayed in the southern part of San Saba county and around the margins of the granite districts in Llano. Near the granite the strata are in places highly metamorphosed and disturbed as in the Colorado, near the mouth of Slick-Rock creek, Burnet county, where they are inclined at an angle of nearly 45°.

The fauna of this division of the Primordial Zone of Texas is very analagous to that of the Potsdam sandstone of Iowa, Wisconsin and Minnesota. I have not been able to discover any species common to the two districts but most of the genera are identical. Thus in the Texan beds we have found *Dikelocephalus*, *Bathyurus*, *Arionellus*, *Conocephalites*, *Agnostus*, *Lingula*, *Discina*, *Orthis*, *Camerella*, *Obolus* and *Capulus*, all of which genera have been discovered in the Primordial sandstones of the northwest except *Agnostus* and *Camerella.*

Compared with the Primordial rocks (Quebec group) of Point Levi, Canada, recently so well described by the Canadian geologists, the Texan strata may be placed on a parallel with the beds provisionally designated as A^2 by Sir W. E. Logan (this Jour., vol. xxxi, p. 216). The propriety of this reference will become apparent by comparing the above-mentioned Texan genera with the following list of fossils given by Logan as occurring in A^2 at Point Levi, viz:—Orthis, Leptena, Camerella, Lingula, Discina, Agnostus, Conocephalites, Arionellus, Dikelocephalus and Bathyurus.*

Descriptions of New Species.

TRILOBITES.

Agnostus Coloradoensis, n. sp.

Head small, nearly circular, convex, length and breadth nearly equal, surrounded with a narrow, neatly defined, flattened border, between which and the dorsal furrows is a small regularly convex surface. Glabella convex, narrow-conical, most elevated posteriorly, occupying scarcely two-thirds the total length of the head, and not quite as wide as one of the cheeks. A single transverse furrow situated near the front divides the glabella into two unequal portions, and at the base on either side is a small triangular lobe. The glabella is strongly separated from the cheeks and front border by a deep furrow.

Length of head, 0·10 of an inch.

A single example only of this species has come under my observation. It bears a close resemblance to *A. orion*, a species recently described by Mr. Billings from the Primordial limestone of Point Levi, opposite Quebec. The *A. Coloradoensis* is, however, a much smaller species, the glabellar furrow is situated much nearer the apex and there is no fissure extending from the glabella to the anterior margin, as occurs in the Canadian species.

Found in Burnet county, near mouth of Morgan's creek associated with *Conocephalites Billingsi* and *Arionellus depressus.*

Arionellus (*Bathyurus*) *Texanus*, n. sp.

Head large, approaching semicircular, wider than long; glabella somewhat regularly convex, conical, distinctly separated from the sides and front by a deep groove; neck furrow distinctly impressed on the sides and nearly obsolete in the middle; border in front of the glabella broad, occupying about one-third the entire length of the head, and marked posterior to the middle with a deep, and rather wide, transverse furrow, which is parallel with the front margin, and has three deep transverse pits; anterior to this furrow is a broad, smooth space sloping from the front margin to the furrow. A line drawn transversely across the middle of the glabella, if extended would pass nearly through to the centre of the eyes. The facial suture passes forward from the inner anterior corner of the eye, nearly parallel with the axis of the body, until it reaches a point opposite the extremity of the furrow of the front margin, thence obliquely forwards and inwards to the front edge; behind the eye it turns suddenly outwards and slightly backwards and cuts the posterior border near the genal angle.

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* The genus *Bathyurus* of Billings appears to me to be closely related to *Crepicephalus* of Owen, and both present many points of resemblance to *Arionellus* of Barrande.

Pygidium short and somewhat massive, subelliptical, axis approaching semicylindrical, elevated above the lateral lobes, as wide as one lateral lobe, and occupying about two-thirds the length of the pygidium; rings four, separated by deep furrows; posterior margin gently arched in the middle and armed on either side with a long, curved, diverging spine (about eight lines long); lateral lobes gently convex, segments indistinct.

Locality.—This is the largest species of the genus I have seen. It occurs in coarsely crystalline limestone of the age of the Potsdam sandstone, at the head of Clear creek, Burnet county.

Arionellus (*Bathyurus*) *planus* (n. sp.).

Head rather large, approaching semicircular, moderately elevated, having a wide border in front, which slopes from the glabellar furrow to the front margin. Glabella convex, oblong, separated from the cheeks and front by a shallow furrow, elevated above the cheeks, sides straight, slightly converging from the neck segment to the anterior extremity; neck furrow shallow, but distinctly defined entirely across; neck segment convex, arched posteriorly and not as high as the glabella. There are several specimens of the glabella of this species in the Texas State Collection, none of which show any traces of transverse furrows.

Length of head, 0·93; length of glabella, 0·54; width at apex, 0·32; width at base, 0·40.

In general appearance this species resembles *Bathyurus armatus* of Billings, but the glabella of our trilobite is strongly elevated above the general convexity, which is not the case in the Canadian species.

Locality.—In the Potsdam sandstone, head of Morgan's creek, Burnet county, associated with *Orthis Coloradoensis*, near the top of the formation.—Texas State Collection.

Conocephalites depressus (n. sp.).

Glabella not much elevated, most convex in the middle, equal to about two-thirds the length of the head, its contour well-defined by the dorsal furrows; sides nearly straight and gradually converging toward the front, which is rounded, lobation indistinctly marked by obscure lateral furrows, neck furrow well defined but not deeply impressed, border in front of the glabella wide, convex, having an elevated marginal rim in front and just within the rim a distinct groove. Surface in front of the glabella marked with very fine, longitudinal, undulating raised lines; no other surface markings visible on any of the specimens under examination.

Length of the head 0·42 of an inch; length of glabella 0·28; width of glabella at base 0·23, at apex 0·16.

This species is closely related to *C. Wisconensis* Owen, specimens of which I have in my cabinet from the Potsdam sandstone of Lake Pepin, Wisconsin. The glabella is, however, proportionally shorter and the front border wider in the Texan fossil.

Locality.—Head of Clear Creek, Burnet county. Texas State Collection.

Conocephalites Billingsi (n. sp.).

Glabella conical, tumid, occupying about three-fourths of the entire length of the head, about one-third longer than wide, outline strongly defined by the dorsal furrows, which are deeply excavated; neck-furrow strongly impressed, lateral furrows three on either side, deeply impressed, the posterior ones curving inwards and backwards from the dorsal nearly to the neck furrow and reaching rather more than one-third the distance across the glabella, middle furrows curved and reaching scarcely more than one-fourth the distance across—anterior furrows quite short, and shallower than the others; neck segment much elevated, convex, having a small median tubercle which is directed obliquely backwards, and has a small central pit, apparently for the articulation of a slender movable spine; front border occupying about one-fourth of the length of the head, margined in front with an elevated rim, between which and the glabellar furrow is a convex surface. Test thin and apparently quite smooth.

Length of head, 0·33 inch, width of base of glabella 0·16.

Of this well marked species of *Conocephalites* we have discovered only portions of the head of several individuals, consisting of the glabella, and immovable cheeks. They were found in gray coarsely crystalline, thin-bedded limestone near mouth of Morgan's Creek, Burnet county.

The species is named in honor of E. Billings, Esq., Palæontologist of tho Geological Survey of Canada.

Dikelocephalus Rœmeri (n. sp.).

Head approaching semicircular, obtusely subangular in front, having a broad border, with a moderately elevated marginal rim, a groove just within the rim, and between this and the glabellar furrow a convex surface. Glabella convex, elevated, and, including the neck segment, occupying not quite two-thirds the total length of the head; sides straight, slightly converging towards the front, which is moderately arched. The transverse furrows are distinctly marked and divide the glabella into three lobes. The posterior furrow is strongly arched backwards and extends quite across, joining the dorsal furrows a little behind the middle of the glabella. The anterior pair curve backwards, reach about one-third the distance across and are situated in advance of the middle. The dorsal furrows are moderately impressed and distinctly limit the sides of the glabella.

Pygidium short, having a broad slightly excavated smooth border, whose outline is convex on the sides and slightly excavated behind, axis prominent, narrow-conical about as wide as one lateral lobe, excluding the border, contracted at apex, separated from the lateral lobes by a distinct but somewhat shallow furrow; rings five, rather wide, separated by shallow furrows, the last two or three of which are indistinct, lateral lobes gently convex, sloping very gradually from thoracic margin to border; segments four or five, wide, flattened convex, the anterior one more elevated than the others, posterior ones nearly obsolete.

An excellent figure of the head of this species is given by Dr. Rœmer in his "*Kreidebildungen von Texas*," (Taf. xi, fig. 2, a), but the author has proposed no name for the species. Fig. 2 c–d of same plate belongs to the head of an *Arionellus*, which has been erroneously referred to the pygidium of this species.

Occurs in crystalline gray limestone, of the age of the Potsdam sandstone, at the head of Clear creek, Burnet county, associated with *Arionellus Texanus*.

Named in honor of Prof. Ferd. Rœmer.—Texas State Collection.

BRACHIOPODA.

Discina microscopica (n. sp.).

Shell very minute, conico-subquadrate, much elevated, nearly as high as long; vertex, nearly or quite marginal, and marked with a small pit; area triangular, flattened, extending from apex to posterior margin. Surface marked with extremely fine, concentric lines of growth.

This is an extremely minute species, the largest specimen being scarcely more than $\frac{5}{100}$ of an inch in length.

A few examples of this species were found in the Potsdam sandstone, near the sources of Morgan's creek, Burnet county. It is associated with *Arionellus Texanus* and *Orthis Coloradoensis.*—Texas State Collection.

Camerella (sp.?).

There are several specimens of a small brachiopod in the Texas State Collection, from the Potsdam sandstone of Morgan's creek, Burnet county, which appear to belong to the genus Camarella recently created by Mr. E. Billings. Unfortunately, however, they consist merely of detached and imperfect valves, too much weathered for satisfactory determination and description.

GASTEROPODA.

Capulus (sp.?).

A small, smooth species of *Capulus* with flattened sides, convex dorsum and narrow curved apex, occurs with the preceding at Morgan's creek, Burnet county, but the characters are not sufficiently distinct to warrant us in giving a description of it.

St. Louis, June 12, 1861.

ART. XXIX.—*On the constitution of the Planetary System: and on the theory and tables of Mars,* (*in a letter addressed to Marshal* VAILLANT); by M. U.-J. LEVERRIER, Director of the Observatory of Paris.*

THE interest which you have taken in the advancement of astronomy and of our knowledge of the system of the world, has induced you to follow with kind regard, my protracted investigations of the four planets, Mercury, Venus, Earth and Mars. It gives me satisfaction to announce to you the completion of these researches;—the last part, relating to the motions of Mars, is finally ended.

The most practical result of these labors is doubtless by comparing theory with observations, to render astronomical tables more precise. Yet the hope of making the tables of the planetary motions absolutely exact, seems insufficient to induce an astronomer to undertake a heavy task of 15 to 20 years, were he not sustained by the thought that he might at least prepare the way for some new discovery:—an ambitious thought, but one which has prompted more than one laborious enterprise. Did not the illustrious astronomer of Königsberg confess that the theory of the Sun had not made the progress which we might have expected from the great number and precision of the observations thereof?

Laying aside long formulæ and tedious calculations, immense series of observations and dry tables of numbers, in short, all professional apparatus, I will here speak only of the results of my labor so far as it relates to the physical constitution of our system.

The existence of the bodies of our system is made known in the most simple manner, when we see them. There is reason to fear, however, that more than one of them may escape us, if, confining ourselves to direct vision as the means of investigation, we do not by some independent method make up for the deficiency of our eyes, even when provided with the most powerful optical apparatus.

The planes of the orbits in which the planets move suffer displacement in the lapse of time, in consequence of the action of masses exterior to the sun; the same is true of the position of the perihelion of the orbits, and the forms of the orbits even undergo alteration. It is evident that the amount of these changes, ascertained by observations, may serve to determine the weight of the bodies which produce them.

Further, were it to be supposed that the only disturbing masses were those of the known planets, it would necessarily result that the variations of all the orbits would agree in giving the same values for these masses; if it shall not, some foreign cause must of necessity have been omitted.

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* From *Comptes Rendus de l'Acad. des Sciences*, of June 3, 1861.

The question presented itself nearly in the following form. Would it be possible by assigning suitable masses to the known planets to satisfy all the observations? Or is there somewhere in our system a notable quantity of matter, which, as yet, has not been taken into account, and of which the consideration is indispensable?

The separate study of each one of the planets affords no answer to this question, whereas the comparison of all the results together enables us to decide with certainty. I will take then the new theory of Mars, and from my previous works, so much as is necessary to the end actually sought, but nothing more.

The position and small size of Mercury and Mars prevent them from exercising any important influence upon the bodies of our system. Observations upon Venus enable us to estimate the mass of Mercury as one five-millionth ($\frac{1}{5000000}$) of that of the sun; while the motion of the earth, deduced from observations of the sun, indicates the mass of Mars as one three-millionth ($\frac{1}{3000000}$) that of the sun. The uncertainty which may exist in these numbers has no influence upon that which follows.

The mass of Venus is about one four-hundred-thousandth ($\frac{1}{400000}$) that of the sun. This result is obtained by several methods; by the consideration of the displacement of the plane of the ecliptic; by the actual measurement of the periodical perturbations of the earth from 1750 to 1810, and from 1811 to 1850; and by the amount of the periodical inequalities of the longitude of Mercury. These results all confirm each other.

The mass of the earth is one three-hundred-and-fifty-five-thousandth ($\frac{1}{355000}$) of that of the sun. This number is derived from a comparison of the force of gravity upon the earth, with the fall of our own planet toward the sun.

These being the data, the theory of Mars may be established by means of them, and compared with meridian observations made a century since, also with observations made upon the near approach of Mars to the star ψ_2 Aquarii, which were made in the year 1672, at Paris by Cassini and Roëmer, at Cayenne by Bouguer who went there to investigate atmospheric refraction, the obliquity of the ecliptic, and the parallax of Mars.

Now, I have discovered that it is not possible to satisfy in this manner, all the observations of the planet; to do this completely, it is necessary to increase the motion of the perihelion of Mars. This increase, if it could be deduced from a change in the received values of the masses of the planets, could not come from a modification of the mass of Venus (for it does not exert a sufficient influence upon the movement of Mars), but only from an addition to the mass of the earth, an addition which it would be necessary to make equal to the *tenth* part of the value given above.

We shall discuss, farther on, the consequences of this result, confirmed as it is, moreover, by a deduction from the theory of Venus. The increase in the latitude of this planet, leads us to a rigorous condition which can only be satisfied by augmenting the mass either of Venus, or of the earth. In publishing the theory of Venus, I had already remarked that the considerations referred to above, would not allow any change to be made in the mass of Venus; that it appeared necessary therefore, to increase by a *tenth* part the mass of the earth; that in view of the importance of this conclusion, it were better, before deducing the consequences of it, to await the completion of the theory of Mars. Now, as we have just seen, the theory of Mars itself demands that we increase the mass of the earth by *exactly* a *tenth.*

I should remark, before proceeding, that Mercury has already given me a result of the same kind. Observation assigns to the perihelion of this planet a more rapid motion than that which corresponds to the masses above admitted. A change of a *tenth part* in the mass of the earth, will not account for this fact; and as it is impossible to increase the received mass of Venus, I have hence inferred the existence of a ring of masses of matter within the orbit of Mercury. The subject has already been discussed, and will be still farther prosecuted; it may not be unprofitable to repeat the language in which I announced my opinion (*Annales,* t. v, p. 105):

"As a mechanical problem we might satisfy the observed transits of Mercury across the sun, on the hypothesis of a perturbing body, whose place is to be determined. It is however indispensable to consider whether all solutions equally satisfy the physical conditions.

"At the mean distance of 0·17, the disturbing mass would be exactly equal to that of Mercury. The greatest elongation which it could attain would be a little less than 10°. Must we believe that a planet shining with a brilliancy more vivid than that of Mercury, would necessarily have been perceived, grazing the horizon, after the setting, or before the rising of the sun? Or is it possible that the intensity of the dispersed light of the sun would have enabled such a star to escape detection?"

"At a greater distance from the sun the disturbing body must be less considerable, and the same would also doubtless be true with regard to its volume, but the elongation would be greater. Nearer the sun, the contrary would be true, and, although the brilliancy of the disturbing body would be increased in consequence of its size and nearness to the sun, it is possible that a star whose position was unknown, might not be perceived in ordinary circumstances."

"But, even in this case, how could a large body having a great brilliancy and situated always near the sun have escaped being seen during some of the total eclipses? Finally, could such a body pass between the solar disk and the earth, and not be recognized?"

"Such are the objections which might be urged to the existence of a single planet, comparable to Mercury as regards its dimensions, and revolving within its orbit. Those to whom these objections appear too serious will be led to replace this single planet by a series of asteroids, the sum of the actions of which would produce the same effect upon the perihelion of Mercury. Aside from the fact that these asteroids would be invisible under ordinary circumstances, their distribution about the sun would prevent their introducing in the motion of Mercury, any periodical inequality of much importance."

"The hypothesis to which we find ourselves conducted, contains nothing extravagant or incredible. A group of asteroids is found between Jupiter and Mars, of which doubtless only the principal individuals have as yet been discovered. There is even reason to believe that the planetary spaces contain an infinite number of very small bodies revolving about the sun. This is certainly true with regard to the region adjacent to the orbit of the earth."

The principal difficulties presented by the system of the four inferior planets, are reduced then to an excess of movement of the perihelion of Mercury, and that of Mars. This two-fold conclusion is worthy of attention; for if the cosmical matter has such a disposition, that although partly or wholly invisible it acts in such a manner as to increase the direct motion of the perihelion, having but little influence apart from this, it is easy to see how the existence of this matter in this condition becomes highly probable.

Now this is, in fact, the mode of action of a series of minute bodies forming a ring about the sun, and revolving from west to east, in the same direction as the other planetary bodies. These bodies, as a whole, could scarcely change the excentricity of the orbit of a planet or cause any sensible periodic inequality in the longitude. Their effect upon the perihelion, however, might become considerable, since it is the sum of the separate effects of each one, so that the final result is sensibly the same as if the whole amount of matter was concentrated in a single mass. Such are the considerations which have induced me to admit the existence of a ring of intra-Mercurial asteroids. The theory of Venus and that of Mars now unite, in confirming these conclusions.

Let us return to the examination of the causes which acting at the other extremity of the system of inferior planets, serve to increase the motion of the perihelion of Mars. They can be accounted for, as we have said, by supposing the mass of the earth increased by a *tenth part.* The motion of Venus in latitude demands the same increase of cosmical matter; but, on the other hand, there is a difficulty with regard to the parallax of the sun.

These demands can all be satisfied and the difficulties all made to disappear by admitting that the asteroids, which, according to observation are situated at the same distance from the sun as the earth have a total mass equal to the *tenth* of that of the earth. This group of asteroids would accelerate the motion of the perihelion of Mars just as an addition of a *tenth* to the mass of the earth would do. If it is situated very nearly in the ecliptic, it will produce the same effect upon the orbit of Venus. It will have, moreover, no influence upon the periodic terms of the perturbations of Venus and Mars. Finally the relation existing between the mass of the earth, gravity and parallax of the sun will not be altered.*

At the beginning, we had hoped that it would be possible to derive the true mass of the earth from the periodical perturbations of Mars, and from its secular perturbations, some knowledge of the total mass of the asteroids situated between Mars and Jupiter.

The first part of this attempt has only half succeeded, on account of the peculiar circumstances of the observations. There was simply reason to believe it unnecessary to increase the mass of the earth. We see at once what high interest attaches to the direct determination of the velocity of light, and, in consequence, of the amount of the solar parallax. We should thus solve a nice question.

The estimation of the total mass of the small planets situated between Mars and Jupiter, becomes impossible if the group of asteroids which is found at the same distance from the sun as the earth is admitted to have any influence. There being no means of separating completely the effects of these two groups, it is only possible to assign the superior limits of their masses, by attributing to each of the groups successively the whole excess of the movement of the perihelion of Mars. It is thus determined, that the whole amount of matter constituting the small planets situated between the mean distances 2·20 and 3·16 cannot exceed about one-third of the mass of the earth.

The constitution of the inferior (or intra-Jovial) portion of our planetary system, deduced from a discussion of observations, may then be summed up as follows:

1st. *Besides the planets, Mercury, Venus, Earth and Mars, there exists between the sun and Mercury a ring of asteroids whose mass is comparable with the mass of Mercury itself.*

2d. *At the distance of the earth from the sun, is found a second ring of asteroids whose mass is not greater than the tenth part of the mass of the earth.*

3d. *The total mass of the group of small planets situated between Mars and Jupiter is not greater than the third part of the mass of the earth.*

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* It is true, that from the determination of the lunar equation of the earth, we have found that both the earth's mass and the solar parallax should be increased. But this result depends on so small a fraction of the lunar equation that we should be fully justified in disregarding it. The final data discussed above have far greater precision.

4th. *The masses of the last two groups are complementary to each other. Ten times the mass of the group situated at the distance of the earth, plus three times the total mass of the small planets between Mars and Jupiter, form a sum equal to the mass of the earth.*

The last conclusion depends upon the determination of the distance of the earth from the sun by observations of the transits of Venus, a determination which astronomers agree in considering as very accurate.

ART. XXX.—*Upon some Improvements, proposed by Sir David Brewster, in the Photographic Camera;* by EDWIN EMERSON, of Troy Univ., N. Y.

SIR DAVID BREWSTER in his "Treatise on the Stereoscope" repeatedly and earnestly insists on the importance of taking the photographic negatives by a lens of very small aperture; he gives as a general rule, (p. 177,) "an aperture as large as the pupil of the eye;" and he makes, also, the following declaration, —"I have no doubt that when chemistry has furnished us with a material more sensitive to light, [than that possessed in 1856,] *a camera without lenses and with only a pin-hole will be the favorite instrument of the photographer;*" in the mean time, however, he suggests that "the use of a lens of rock-crystal, which has a low dispersive power, and having the ratio of the curvature of its surfaces as six to one, with an aperture of one-quarter of an inch, would make an approximatively perfect camera." (pp. 137, 138).

Being convinced, by the excellent results obtained by myself, that a small aperture was indispensable to success in point of sharpness, which was a confirmation of Sir David Brewster's main idea, and having been furnished, by my friend Prof. Rood, with an instantaneous collodion, I began to test by careful experiments, Brewster's notion of a pin-hole camera, and also the use of a quartz lens for the camera, I also endeavored to determine approximatively how small the diaphragm of the ordinary photographic camera should be reduced so as to secure the best results. The instrument in my possession is a very excellent stereoscopic camera, fitted with two portrait combination tubes, and the best achromatic lenses, of Harrison's make. The results were either obtained by this camera or were compared with its work.

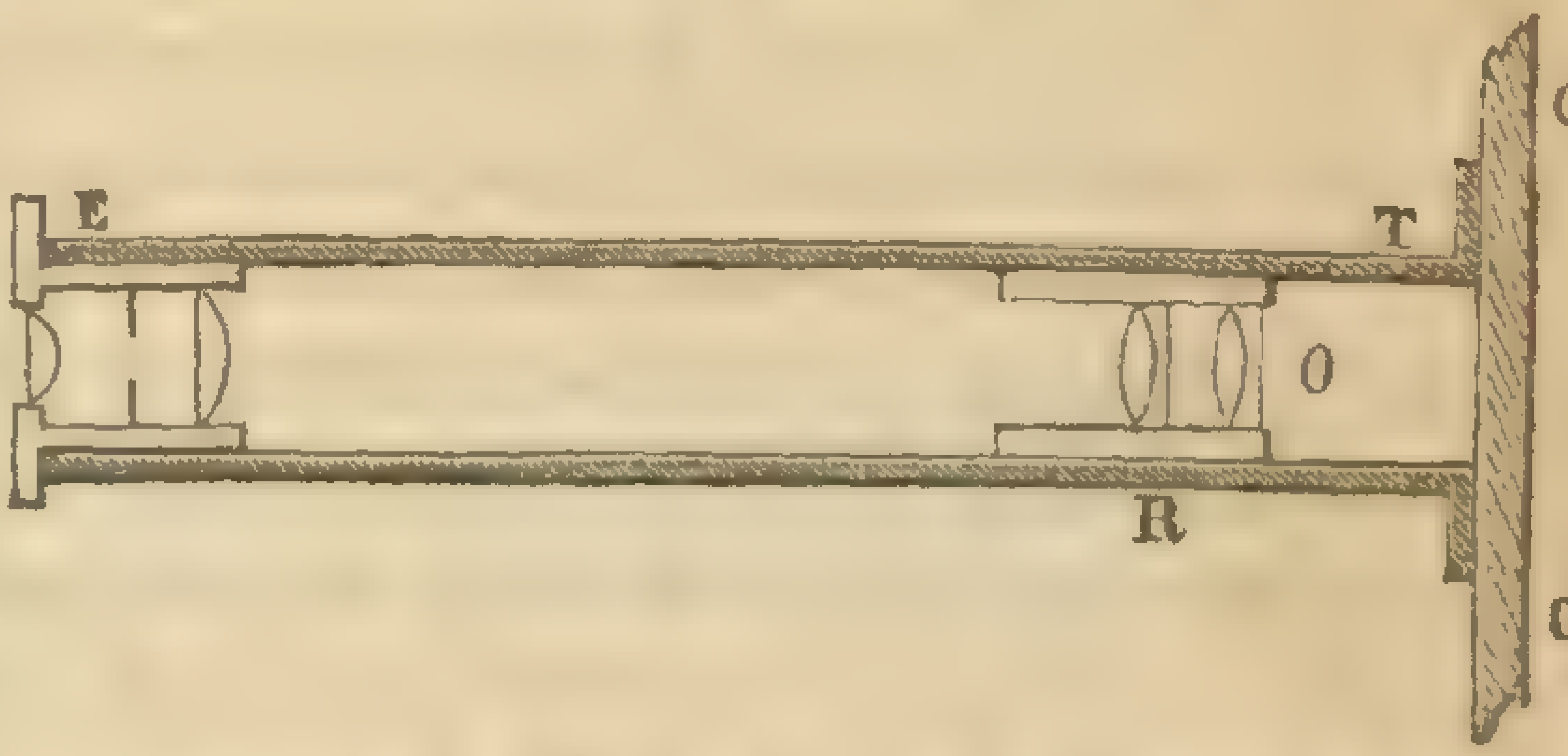
Photographers generally focus upon the ground glass of the camera with the unaided eye, and, of course, in a majority of instances get only an accidental approach to a perfect focus; and when the aperture is over half an inch, for lenses of 6 or 8 inches focus, only a small portion of the whole picture is in focus at all.

Those who use a magnifying glass and a smaller diaphragm succeed better, but by neglecting to reduce the diaphragm to a minimum, their results are by no means as perfect as they might be. By the aid of a common magnifying-glass one and a half inches in focal length, with which to view the image on the ground glass of the camera, and an aperture of two-fifths of an inch in the diaphragm in taking the negative, I was able without any difficulty, to obtain results fully equal in point of sharpness, to the best French transparent views. But in attempting to improve upon this, by reducing the aperture and by increasing the power of the focusing-lens, two difficulties were experienced,—1st. The light was so much diminished that it was well-nigh impossible to see minute shades of difference in the sharpness of definition, and, 2d. The inequalities of the finest ground glass were so much enlarged as seriously to obscure and distort the view projected upon it. This last proved an insuperable bar to any higher degree of excellence by these means, as a single irregularity in the ground glass was magnified quite enough to obscure many fine details. Moreover, with a small diaphragm of the size recommended by Brewster, the light was so faint that a variation of half an inch in the distance of the lenses of the camera from the ground glass, produced no *perceptible* variation in the sharpness of the image. A series of experiments was then made with two objects in view,—1st, to carry up the power of the focusing lens, and, 2d, to subsitute for the ordinary ground glass of the camera something of finer texture so as to stand the microscopic enlargement, and of more transparent quality so as to increase the light. An ordinary "Fothergill plate," covered with the delicate yellowish film of iodid of silver enabled me to carry the magnifying power to fifteen diameters with an aperture of one-fifth of an inch. At the suggestion of Mr. Grunow, of New York, a plain glass plate, lightly covered with dust was tried, which with a power of fifteen diameters rendered practical the employing of an aperture of three-fifths of an inch. But in all the experiments with a single microscope the focus was too indefinite and uncertain for the accurate work I had in view, although the sharpness obtained was far beyond anything I had seen produced by the professional photographers. Indeed, among both British and American artists and amateurs in photography there seems to be a wide difference of opinion as to what may be regarded as *a standard* on this important point. I have not seen a suggestion of the proper means of determining it; which is, undoubtedly, an appeal to the compound microscope.

At this stage of the investigation Prof. Rood constructed for me a compound microscope of a power of thirty diameters, which I hoped to be able to employ as a focuser. Its form and construction will appear from the following diagram, fig. 1.

This instrument I had intended to use upon the focussing plate of plain glass delicately covered with dust; but it occurred to me that possibly plain glass alone would answer to focus upon, and if so, all the obstruction caused by magnified particles of dust, and the want of sufficient light caused by the opacity of the Fothergill film, or anything of a similar character, might be entirely overcome. A trial showed that the amount of light was very greatly increased; but I was now unable to determine the position of the image projected by the lenses of the camera. The lenses of the camera in connection with the lenses of the microscope formed a telescope of considerable power, the lenses reciprocally supplementing each other, so that no matter where the image produced by the lenses of the camera might be formed in space, the microscopic focusser could be readily adjusted to suit it, and give a perfect view. To obviate this uncertainty, fine parallel lines were drawn by a diamond, one-twentieth of an inch apart, upon the surface of the plain focussing-glass nearest to the lenses of the camera; the microscope was then carefully adjusted to distinct vision of these lines, and its lenses fixed in that position; it was now easy to cause the image formed by the camera to coincide in position with these parallel lines, and thus the perfect physical focus for the sensitive plate was absolutely determined, allowance, of course, being made for any difference between the chemical and visual foci, which this arrangement afforded a means of adjusting with the greatest accuracy.

1.



G, Polished plate glass substituted for the ground glass, having fine lines drawn by the diamond on the surface farthest from the microscope.—O, Achromatic objective.—E, Eye-piece.—R, A rack-work at R moves the objective in and out.—ET, Exterior tube with a projection at the objective end at right angles, so as to apply it accurately ro the flat surface of the glass.

By this method of focussing very small objects, such as a single leaf of an elm tree over half a mile distant, were distinctly visible, and were readily focussed upon; and, also, the difference in the focus of objects at a mile, and those at a mile and a half in distance was made apparent; which is utterly beyond the power of the single microscope. The use of a very small diaphragm enabled me now to secure the finest details in all the objects visible in an extended landscape, so that they would bear examination under the compound microscope with a power of 150 to 200 diameters; or, in other words, with a power as high as the structure of the collodion would bear. Up to this time I had supposed that the lenses of my camera were of the same focal length, as very delicate manipulations had failed to detect any discrepancy between them; but by this method of focussing, a variation of nearly one-fifth of an inch was detected immediately. By the use of the instrument the aperture was reduced to one-tenth of an inch, which from careful experiments I regard as about the minimum for good results, for lenses of six inches focus.

I was now prepared to test the ideas of Sir David Brewster, 1st, with regard to a small aperture, 2d with reference to a rock-crystal lens, and 3d, as to a pin-hole alone substituted for a lens, in connection with a very rapid collodion. By means of the binocular camera I was able to make two experiments simultaneously, on the same plate, the left-hand view being taken by the portrait combination, and the right-hand view being taken with a quartz lens, or a common glass lens, or a pin-hole, as the case might be. The scenes or objects photographed were always exposed to the full light of the sun; and the plate, in each pair of experiments, was, of course, subject to a uniform treatment, under similar conditions as to collodion, baths, &c. I was thus able to compare the results of the various experiments side by side with the action of the achromatic combination; as follows—

Portrait combination, 6 inches focus.	*Quartz lens, 6 inches focus.*
1. One second exposure, $\frac{1}{4}$ inch diaphragm. A pretty fair negative.	1. One second exposure, $\frac{1}{4}$ inch diaphragm. Action slightly quicker, negative pretty fair, but less sharp.
2. Thirty seconds exposure, $\frac{1}{4}$ inch diaphragm, good negative, would stand a magnifying power of 30 diameters.	2. Thirty seconds exposure, $\frac{1}{4}$ inch diaphragm. Action slightly quicker, more contrast, but would not stand a magnifying power of 5 diameters.
3. Two minutes exposure, $\frac{1}{10}$ inch diaphragm, strong negative, view sharp all over the field, would stand a magnifying power of 100 diameters.	3. Two minutes exposure, $\frac{1}{10}$ inch diaphragm, no quicker action, no stronger negative, and would not stand a magnifying power of 5 diameters.
4. Two and a half minutes exposure, $\frac{1}{12}$ inch diaphragm, negative good, but would not stand a power of over 50 diameters.	4. Two and a half minutes exposure, $\frac{1}{12}$ inch diaphragm, no quicker action, negative would only stand a magnifying power of 4 or 5 diameters.
5. Pin-hole diaphragm, could see nothing distinctly to focus upon, four minutes exposure, very faint impression on the sensitive plate.	5. Pin-hole diaphragm. Similar result.
6. Pin-hole diaphragm, seven minutes exposure, strong negative, but no fine details.	6. Pin-hole diaphragm. Similar result.
7. Common glass lens, 6 inches focus, $\frac{1}{10}$ inch diaphragm, two minutes exposure, view sharp, would stand 30 diameters, tolerably.	7. $\frac{1}{10}$ inch diaphragm, two minutes exposure, quartz action quicker than the glass, more intense negative, but not as sharp, owing possibly to a slight double refraction in the lens.

Portrait combination, 6 inches focus.	*Pin-hole alone.*
1. One second exposure, one inch diaphragm, a good negative.	1. One second exposure,—result nothing.
2. Thirty seconds exposure, half inch diaphragm, a good negative.	2. Thirty seconds exposure, result almost nothing.
3. One minute exposure, $\frac{1}{4}$ inch diaphragm, good sharp negative.	3. One minute exposure,—faint shadowy outline of a picture.
4. Two minutes exposure, $\frac{1}{10}$ inch diaphragm, excellent negative, details of objects with a mile good under a magnifying power of 100 diameters.	4. Two minutes exposure, middling contrast between light and shade but exceedingly blurred in all the details.
5. Three minutes exposure; picture over-exposed, with $\frac{1}{10}$ inch diaphragm.	5. Three minutes exposure, view of a white bust in the sun-light, good contrast of light and shade, picture only of the prominent outlines looked like a very rough mosaic.
6. Three minutes exposure, $\frac{1}{12}$ inch diaphragm, good negative, but not sharp enough for a power of 50 diameters.	6. Seven minutes exposure, negative strong enough to print from; but no sharpness at all; details blurred and confused.

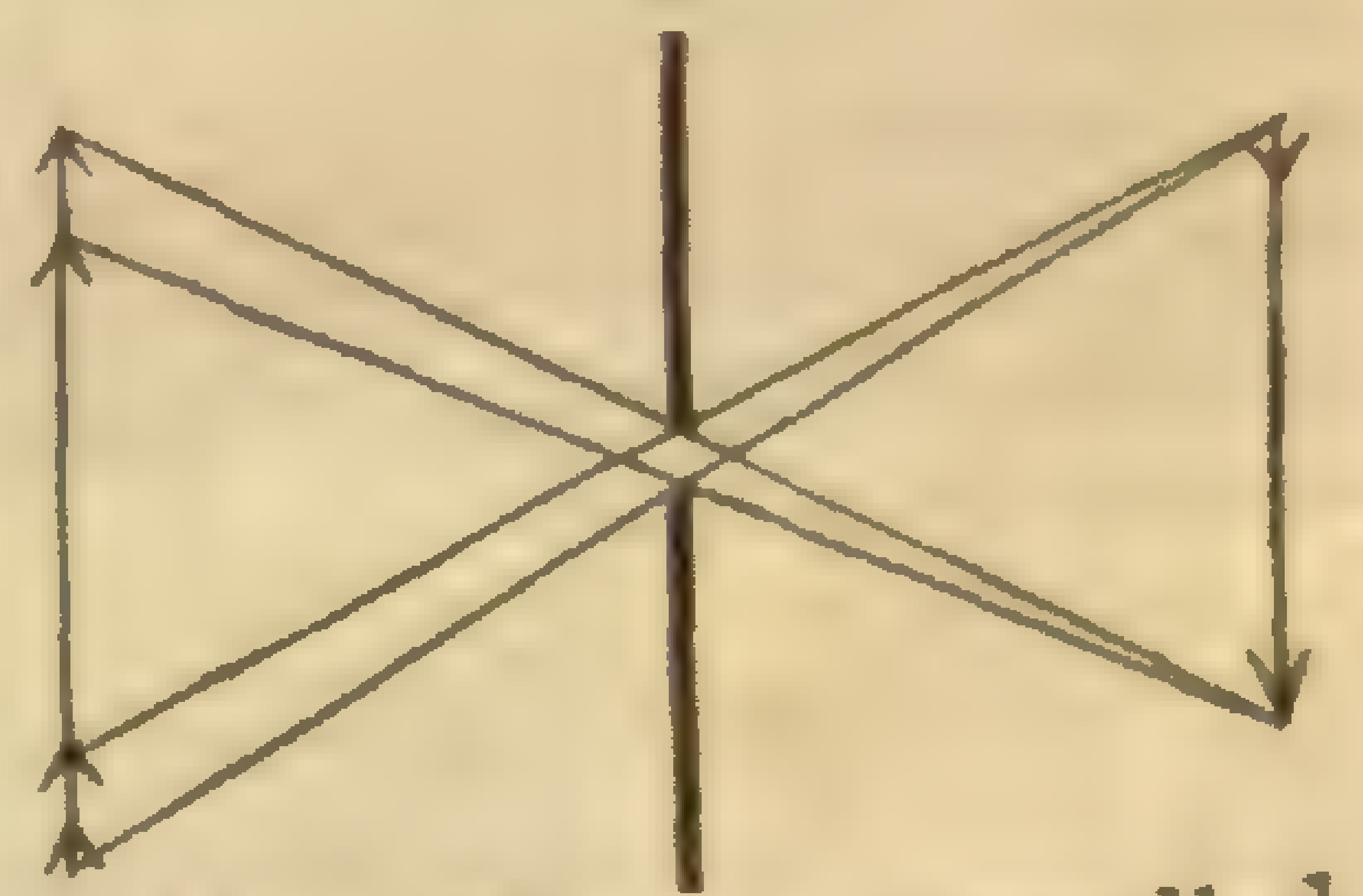
The results of these experiments may be summed up as follows:—

1. It is found in practice that a quartz lens is quicker in action than a common glass lens of the same aperture, focus and thickness.

2. A quartz lens is very slightly quicker than the double combination of achromatic lenses; but it will not afford any thing like so sharp details even under the smallest diaphragms. "A quartz lens camera with $\frac{1}{4}$ inch aperture" is, therefore, very far from being "an approximatively perfect camera."

3. The simple pin-hole will not compare under any circumstances with the commonest glass lens; as it gives no sharpness to the picture, and requires from three to seven minutes exposure, with the most sensitive collodion. This lack of sharpness might have been predicted theoretically. To give sharpness to the image, the pin-hole must be as small as the smallest detail in the view required, say the ten-thousandth of an inch, otherwise the rays of light from a single point travelling such paths as are indicated in the diagram produce necessarily a blurred image; thus—(fig. 2,) i. e., sharpness will be in proportion to the smallness of the pin-hole; now as a pin-hole of the $\frac{1}{50}$th of an inch in diameter requires with the most sensitive collodion an exposure of several minutes, it is apparent that a hole the ten-thousandth of an inch in diameter would require an exposure entirely impracticable. We are compelled,

2.



therefore, to regard Sir David Brewster's pin-hole camera as an optical as well as a photographical absurdity.

4. The diaphragm for lenses 6 inches in focus, can be reduced down to about an aperture of one-tenth of an inch with a very decided improvement in sharpness, if the compound microscope is used to focus with on a plain glass focussing plate; but when we reduce the aperture below this there is a loss of sharpness; owing, it may be, to the approximate parallelism of the rays.

5. A good use for a pin-hole camera would be as a simple instrument for testing the sensitiveness of collodions, and I would recommend it for this purpose.

Troy University, August, 1861.

ART. XXXI.—*On the Age of the Red Sandstone formation of Vermont;* by E. BILLINGS.*

I HAVE lately been examining a tract of the Calciferous sandrock which lies on the boundary line between Canada and Vermont on Missisquodi Bay. The rock is exposed here in long parallel ridges, over an area of eight or nine miles in length and from one to three in width. On the east side of the exposure there is a ridge of greyish sandstone which I traced south across the boundary line, after crossing which it soon becomes interstratified with thick beds of rock of a chocolate red or brown color. It is here the typical red sandrock formation of Prof. Adams. Hearing that Dr. G. M. Hall and Rev. J. D. Perry of Swanton had discovered trilobites near this place I called upon them and they kindly conducted me to the locality. It is above two miles south of the line and one mile or a little more east of the Highgate Springs. The individual fossils are abundant in the red sandstone but I could find only two species, a small *Theca* and a *Conocephalites*. Of the latter we found only the head but the specimens are very numerous and some of them well preserved. The species resembles Bradley's *C. minutus* but is a little larger and I think quite distinct therefrom. It is a true primordial type and if we are to be guided at all by Palæontology we cannot regard this rock as lying at the top of the Lower Silurian but at the very base of Barrande's Second Fauna if not indeed a little lower. It is therefore not the Medina Sandstone but a formation somewhere near the horizon of the Potsdam. This accords exactly with conclusion drawn from the evidence afforded by the fossils discovered by our survey at Quebec last year.

Museum of the Geological Commission, Montreal, Canada, July 22, 1861.

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* In a letter to one of the Editors of this Journal.

ART. XXXII.—*Agricultural Chemistry—Soil-analysis: Notice of the Agricultural Chemistry of the Geological Surveys of Kentucky and Arkansas*;[*] by Prof. S. W. JOHNSON of Yale College.

IN no country has there been so much popular appreciation of practical science as in the United States of America. Scarcely one of the States is without its volume or volumes of Geological and Natural History Reports, and though some of them have been content to confine the work to the merest outline of the general and industrial geology of their territory, and have expended but a few hundreds of dollars in the undertaking, others, like New York, have embraced all the branches of Natural Science in their survey, have prolonged the work of exploration or elaboration through many years, and have devoted money to these objects with unsparing hand.

The results of these surveys as they stand recorded in the numerous volumes published by the States and by the General Government, are of very unequal merit, as might be expected from the wide range of country explored, from the various degrees of interest and appreciation governing the many Legislatures which have authorized these labors and from the exceedingly unequal ability of the individuals charged with their execution.

These explorations have originated in all cases with our scientific men. It is their influence either brought to bear immediately upon the legislative bodies, or exerted less directly through cultivated and public spirited persons to whom the possible benefits of geological surveys have been explained—that has accomplished this vast work.

The enterprises of which we speak being sustained pecuniarily at the expense of the people, and depending from year to year in many cases upon the popular vote, it has been not only politic but right to exhibit at the outset the prospects of pecuniary return for the required outlay of means, as an inducement to support such undertakings. It has been no less proper in presenting the results of the surveys, to lay stress on the discoveries having industrial bearings which are the fruits of the work.

In those States where large quantities of metallic ores occur, the interest of capitalists engaged in mining has often sufficed to inaugurate a geological survey. In other states the agricultural sentiment has had to be operated upon.

Great results have been promised to agriculture from the applications of geology and chemistry, and a great deal of labor has been performed in the attempt to satisfy the hopes that have been thus excited.

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[*] 1st, 2d, 3d and 4th Reports of the Geological Survey of Kentucky 1854-60: 2d Report of the Geological Reconnoisance of Arkansas, 1860: Agricultural Chemistry and Geology by Dr. D. D. OWEN, principal Geologist, and Dr. ROBERT PETER, Chemical Assistant.

The chief object of the present notice is to inquire what has been really accomplished for the good of the farmer, by the scientific surveys that have been hitherto prosecuted in this country.

The labors of Dr. Peter in connection with the Kentucky and Arkansas Surveys being the most recent and extended attempts of this kind, we shall make them the basis of our inquiries.

If we except a few pages of general remarks on the theory of vegetable nutrition, &c., which while useful to the practical readers of the Report contain no new facts or principles,—the whole effort of Dr. Peter has been concentrated on the analysis of soils, marls, rocks and ashes. He publishes in the four Kentucky Reports analyses of 375 soils, and in the Arkansas Report, 187, in all 562 soil analyses. Besides, we find the results of examinations of 145 rocks, shales, &c., and of 38 ashes of plants, making a grand total of 795 agricultural analyses.

The agricultural fruits of the surveys of Kentucky and Arkansas are then to be sought in these analyses.

It certainly will strike all that the amount of work performed by Dr. Peter is unusually great. It is now but six years since the Kentucky survey was commenced and in that time the Dr. has not only analyzed 795 soils, but has executed 516 analyses of ores, slags, mineral waters and coals, making an average of two analyses for every three days of this whole period. This labor Dr. Peter states he has accomplished with the help of one intelligent assistant, and by a special organization of his laboratory and his operations whereby the utmost economy of time was secured. We have had such experience of the advantages of a similar system, that we are not prepared to doubt that the chemist who adopts a plan of analysis which fully satisfies him, and from which he never departs, may execute such an amount of work. At the same time we must bear in mind that the only control Dr. Peter offers for the accuracy of his results is, that the sum of the weights of the separated ingredients equals their original conjoined weight, no time being allowed to repeat a determination, or to prove the purity of a precipitate.

The Analytical Process followed in these analyses is not by any means so minute and full as we should be warranted to expect, when their author declares (4th Ky. Rep., p. 57) that "such a work to be eminently useful must be thorough and exhaustive;" for soluble silica, chlorine, nitric acid and ammonia are not at all estimated, and the condition of the iron, whether protoxyd or peroxyd, is not noticed. It is worthy of notice that carbonic acid and lime are always present in atomic proportions in the soils latterly analyzed, no excess of either ingredient being mentioned in the results. Carbonic acid however is not noticed in the description of the analytical process, and that figuring in the analyses does not appear to have been directly estimated, but to have come from the oxalic acid of the reagent shelf.

If, as might easily happen, the contrary not being proved, a portion of the lime dissolved by hydrochloric acid exists in these soils as silicate, sulphate or phosphate, then the assumption that it is united to carbonic acid introduces an error into the summing up (which in many cases is exactly 100) and shows that a quantity of some other ingredient has been overlooked.

For the estimation of phosphoric acid a highly modified form of Sonnenschein's process is employed, but our author does not give the figures which prove that his changes are improvements.

Admitting however that the analyses are correct—we next inquire what is their value—what useful deductions from them appear in these Reports.

In the introduction to Vol. i, Kentucky Survey, page 13, Dr. Owen says: "By consulting the numerous interesting results obtained by the chemical analyses of the soils embodied in the pages of this report, abundant evidence will be gathered of the vital necessity of wide dissemination amongst the farming community, of the knowledge to be obtained by a correct insight into their chemical constitution." In the same volume, page 373, Dr. Peter remarks that he was impressed "that when the composition of our Kentucky soils and minerals in general, is once accurately established, their applications to our wants and uses would be obvious to all well informed persons. He has therefore consumed the time mainly in the analyses, and made up his report principally of the results."

In the agricultural section of the Arkansas Survey, p. 47, Dr. Owen says:—"principally from chemical soil-analyses can the agriculturist form an intelligent opinion as to the comparative fertility of soils, and their suitability to the growth of certain plants, as well as judge what applications may be required in the way of lime, bone earth, plaster of Paris, ashes, or salts of potash, soda, &c."

Dr. Peter, in the same volume, page 166–7, observes:—"It is believed that by no other mode than by chemical analysis or by the more tedious and laborious method of actual experience, in cropping for a series of years and publishing a record of the same, can the actual nature, capabilities and value of the various soils of a State be presented to the public; and that by instituting this Geologico-Agricultural Survey, the State of Arkansas not only aids materially in the progress of the general science of the civilized world, and that of the soil in particular, but takes the most effectual mode of making known to the enlightened immigrant her agricultural riches. In this she has followed the wise lead of the older state of Kentucky, in which, since the institution of her geological survey, the value of the land in the regions examined and reported on has been very greatly enhanced."

In the Agricultural Geology of Kentucky, Report 2d, p. 9, Dr. Owen says: "Placing implicit reliance on the capabilities of chemical science to indicate by the analyses of soils, the ingredients removed by the cultivation and harvesting of successive crops, it was hoped that by collecting samples of the virgin soil, and of the same soil from an adjacent old field, that not only the different substances assimilated out of the soil could be ascertained, but also the exact proportion of these so that the farmer might know precisely what must be restored to the land to bring back its original fertility."

These quotations sufficiently show what were the opinions which led our author to devote such an amount of labor to the analysis of soils, and indicate in general, what results were expected.

In the 2d Arkansas Report, p. 49 et seq., Dr. Owen "proceeds to explain in what way soil-analysis becomes of value to the farmer." He desires "to call particular attention to this subject, because the opinion has been expressed even in this year (1860), and by those having a high standing in the scientific world, that chemistry is incapable of conveying any useful information to the farmer by analyzing his soil."

On the six following pages of the 2d Ark. Rep., and on page 30 of the 4th Ky. Rep., Dr. Owen gives the most complete *résumé* of the teachings of soil-analysis which we are able to find in the five volumes before us, and as these are his latest writings on the subject, and as he then had the data of 389 analyses, viz. of 187 Arkansas soils and 202 in the three volumes of the Kentucky Report,—these being refrered to on the pages we are quoting from,—we are warranted in considering what he has here presented, as embodying the *strong points* in favor of soil-analysis. We will notice them separately as gathered from both Reports.

1st. "Any one who will take the trouble to inspect the analyses of the 187 Arkansas soils will see that the relative proportions of the eleven mineral constituents of these soils is very accurately given."—*2d Ark. Rep.*, p. 49.

If we admit fully that Dr. Peter's analyses represent with fair accuracy the composition of the two grammes of soil he experimented with in each instance, we do not therefore allow that the composition of "these soils" considered as representing geological formations, or large agricultural districts, or even single fields, is "very accurately given."

Here at the outset the distinguished gentlemen who have conducted the 'geologico-' and 'chemico-agricultural' part of the Kentucky and Arkansas surveys have taken for granted, what being an error, overturns their whole reasoning, and renders their soil-analyses comparatively worthless.

Years ago, following the teachers of agricultural chemistry in this country and England, we believed that soil-analyses were adapted to be of exceeding use to farmers. Having practised analytical chemistry sufficiently to undertake the work, we proceeded, when on a vacation visit, to collect some farm soils for the purpose of applying our skill and knowledge. On putting down the spade and post-augur into the drift overlying the lowest Silurian of Northern New York, we were at once struck with the difficulty of procuring an average specimen. The soil for a depth varying from two to six inches was quite fine, but below that depth largely mixed with gravel. On comparing different samples taken from a small area, it was plain that the soil was not a fit subject for analysis. The relative quantities of organic matter as indicated by the color of the surface of small stones,—some quartz and granitic, others slate and limestone of several geological members,—were astonishingly variable. Here we found the soil sandy, there it was clay. To take a sample from one place was to do obvious injustice to the sixty-acre field. To take it from a dozen places would not render the selection of a fair sample any more certain. Then as to depth —was it proper to go down six inches, one foot, or how far? Had the field been a bed of iron ore, assays of a dozen samples taken from different parts would have indicated very satisfactorily the general value of the deposit, would have served as data for buying and selling the property, because the worth of an unworked bed of such ore depends less upon its content of iron than upon external circumstances which affect the extracting of the metal. Had the field been covered with rich dressed copper ore to the depth of six inches, it would have been necessary to divide it up into small parcels of a few tons, average these carefully and as carefully assay each one. No one would risk purchasing a hundred thousand tons of copper ore on the analysis of one or of a dozen samples, because it is impracticable to intermix or average such a mass of material as that a dozen samples shall accurately represent it.

We hold it therefore as the first objection to soil-analyses that to procure a specimen which accurately and *certainly* represent a field or district, is practically impossible in the majority of cases, *and if possible, requires a series of analyses to prove the fact.*

This argument applies with the greater force when we consider how small a proportion of the ingredients of a soil are of any immediate use in feeding crops. The really active nutrient matters of a soil are not reckoned by per cents nor by tenths of per cents, but by the minutest fractions.

A heavy crop of thirty-seven bushels of wheat, grain and straw included, removes from an acre of land but 300 lbs. total of mineral matters. According to Dr. Peter's weighings on some of the Kentucky soils, we may assume, that taken to the depth of a foot, an acre of soil weighs 3,000,000 lbs. All that is removed by the heaviest wheat crop then in one year is but $\frac{1}{30,000}$, or 0·0033+ per cent.

It follows that the annual removal of the heaviest crop of wheat from a soil for 100 years diminishes its mineral matters by less than 0·4 per cent. If then, in the selection of a sample, the average composition is departed from to the amount of 4 parts in 1000, the analysis may misrepresent the soil, by the value of 3700 bushels of wheat per acre, or by what represents, so far as mineral ingredients can, the fertility of a century.

What freaks and accidents is not the soil-analyst the sport of? A bird, squirrel, or dog relieving nature at the spot where he collects his sample, innocently magnifies the phosphoric acid or alkalies of the surrounding thousand acres a hundred fold. The soil gathered toward the end of a long rain, whereby its soluble matters are carried deep into the subsoil, is declared poor, by analysis, whereas if taken after a fortnight of drought it might appear extraordinarily fertile. Boussingault found in his rich garden soil in June, during wet weather, 0·00034 per cent of nitric acid. In the following September, after a period of dryness, it contained 0·0093 per cent, or twenty-seven times as much as in June. This ingredient is indeed more liable to fluctuation in amount than any other, both because it is formed in the soil, and because it is not subject to the absorbent action which the soil exercises over most other of its soluble constituents; but the same variation occurs among the other ingredients according to the direction of the capillary movement of the soil-water, though in less degree.

Independently however of all considerations and calculations like the above, we have proof—evidence at least that supports these considerations, and has never been publicly refuted—that it is practically impossible to obtain average specimens of the soil. I refer to investigations made as long ago as 1846–9 under the direction of the Prussian "*Landes Oekonomie Collegium*," and reported by the distinguished Magnus. The account of these experiments is given in detail in Erdmann's Journal für Praktische Chemie, vol. xlviii, pp. 447 et seq.

The "*Landes Oekonomie Collegium*" at that time carried on systematic experiments in agriculture at fourteen distinct stations scattered through the Prussian domain. The trials which we now speak of, were made for the ostensible purpose of studying the exhaustion of the soil by cropping. The plan was to analyze the fourteen soils, the history of which for years previous was accurately known, then crop them with rape until "exhausted," then compare together the original composition of the soils with their composition after exhaustion, taking into account as well, the composition of the crops removed. The research began with collecting and analyzing the soils. In order to meet as far as possible the difficulties of securing average specimens, equal portions of the soil of each field were taken with the spade at ten or twelve different points, and thoroughly intermixed; each sample was then passed through a sieve, the holes of which were two square lines in area, so as to remove all coarser stones, then again well worked over to complete the mixture. Of each sample three separate portions were analyzed, in most cases by different operators. The analyses were made by, or under the guidance of, the ablest chemists of Germany. They were made according to a prescribed scheme, and that there should be no reason to slight the work, the work was paid for. It is true that analytical chemistry was not so advanced in 1846 as now. It is true that the methods then practised for estimating phosphoric acid and some other substances were not as perfect as they now are; but for the most part the analyses then made are as accurate as any that could be executed to-day. It cannot be supposed for a moment that analysts like Rammelsberg, Bödecker, Genth, Debus, Knop, Heintz, Krocker, Marchand, Weidenbusch, Sonnenschein, Varrentrap, Weber, &c. &c., would by fault of method or by carelessness return anything but results that were accurate, as far as it was possible to make them such. We cannot suppose that their determinations of lime, oxyd of iron, potash and sulphuric acid, substances estimated then by the same methods that are now employed, would vary if they were supplied with homogeneous material to operate upon. But let us look at some of their figures. We tabulate a number of them taken at random:

Soil of		Lime.	Potash.	Sulphuric acid.	Phosphoric acid.
Eldena,	*a.*	0·39	0·93	0·08	0·06
	b.	0·75	2·06	—	0·17
	c.	0·25	0·12	0·02	0·40
Beesdau,	*a.*	0·802	3·825	—	0·042
	b.	0·039	0·490	—	0·046
	c.	0·715	0·792	0·004	0·007
Neuensund,	*a.*	1·692	3·531	0·050	0·051
	b.	0·614	1·289	0·038	0·010
	c.	0·728	1·243	0·241	0·121
Turwe,	*a.*	2·312	1·112	0·040	0·057
	b.	2·67	1·14	0·03	0·20
	c.	3·391	0·201	0·022	0·014
Frankenfelde,	*a.*	0·420	1·155	0·016	0·004
	b.	1·081	—	—	0·418
	c.	0·461	1·456	0·015	0·071

If we run over these figures and those of the entire series of analyses, we find that different determinations disagree to such an extent as to make it the sheerest folly to base any calculation of the value of the soil upon analysis. Some of the analyses agree sufficiently to show that accordant results are possible if uniform material be taken; but the grand result of the investigation is that the difficulties of getting a uniform material are exceedingly great. Again, we must remember that in the case before us, the three examinations of each soil were made upon portions of one carefully mixed sample. What would have been the result had each chemist received a sample collected separately from all the others, and from different parts of the field!

Dr. Peter mentions these analyses of the *Landes Collaeium*, and quotes a few of the results on page 187 of the 3d Kentucky Report. He believes however that these discordant results do not invalidate soil analyses when made as they may be made with "means and appliances *now* at the service of the analytical chemist" and thinks "this statement however hazardous it may seem will be found to be sustained" in his Report.

In the Report before us however we do not find anything to sustain Dr. Peter's view. He gives, so far as we have discovered, no duplicate analyses, to show what accuracy his methods admit of on the same sample, much less does he prove by analyses of specimens separately gathered from the same field, that it is easy to procure an average material for analysis. Until this proof is produced the evidence is in favor of our view.

Having shown how small an error in sampling may affect the chemist's estimate of a soil, it is not out of place to insist for a moment, that a similar error in the analysis itself, must have the same result. In running over 200 pages of Dr. Peter's 4th Kentucky Report, we find five analyses of soil in which there is a gain of from five to eight tenths per cent; we find twenty-three in which there is a loss exceeding five tenths per cent. In thirteen of the latter the loss is eight or more tenths, in eight instances the loss is one per cent or more, and in one case is one and eight-tenths per cent. We should scorn to notice little matters like these, errors which are inseparable from the best manipulation and the best processes, were it not that in soil analysis it is precisely the small quantities which alone have any importance.

We find in Dr. Peter's work, as in the work of all who have preceded him in the analysis of soils from Davy and Sprengel down, evidence that the best endeavors in this line of research are entirely incommensurate with the desired results.

It may be objected to this criticism of the analyses that the loss or gain must be distributed among the twelve ingredients determined. It is true that there is a probability that such distribution would be just; but this is by no means *certain*, and it is equally true that this being done there is still force in the criticism—for the four-tenths per cent of the soil which a century of wheat crops would remove, likewise consists of twelve ingredients.

The 2d result of these analyses, according to Drs. Owen and Peter, is what the former (4th Ky. Rep., p. 33) declares to be "a general law" "now established," viz., "that *soil-analysis is capable of showing the exhaustion in land of the mineral food of plants by continual cropping.*"

To show the removal of soil-ingredients by cropping, the plan was followed of collecting soils from contiguous fields, one of which had been "cultivated" while the other was in its virgin state. On comparing the analyses it was found that in seventy-one[*] cases out of seventy-nine, a loss had occurred in the soil which had been in use without manure from ten to fifty years. In eight instances, however, the analysis failed to show such a result, owing to local causes, the soil of the old field being based on a sub-soil richer than was the virgin field, or the old field having received washings of more elevated lands, &c.

The admitted richness of the old over the new soil in these eight exceptional cases, is expressed by hundredths of per cent, e.g., soil Nos. 982, virgin, and 983, cultivated, differ by 0·066 per cent of potash. Soils 1144 and 1146 by 0·032 per cent of phosphoric acid. Soils 1204 and 1205 by 0·092 per cent phosphoric acid. Soils 1207 and 1208 by 0·033 per cent potash. Similar fractions likewise show the amount of deterioration in the other seventy-one cases. We adduce two instances pointed out by Dr. Peter in the 3d Kentucky Report, p. 207, and one given on p. 176 of the 2d Arkansas Report:

	Carb. of lime.	Magnesia.	Phosphoric acid.	Potash.
Virgin soil, No. 557,	0·345	0·335	0·181[†]	0·156
Old soil, No. 558,	0·215	0·465	0·103[†]	0·101
Difference,	0·130	0·130 gain.	0·078[†]	0·055
Virgin soil, No. 738,	0·180	0·444	0·179	0·256
Old field, No. 739,	0·145	0·388	0·163	0·179
Difference,	0·035	0·056	0·016	0·077
Virgin soil, No. 288,	0·121	0·371	0·127	0·116
Old field, No. 289,	0·021	0·371	0·053	0·097
Difference,	0·100	0·000	0·074	0·019

We were prepared to find these differences much larger. It is seen at a glance that they fall within the errors of Dr. Peter's own manipulation, and when we assert that of ten analyses of the most homogeneous material made by the same analyst under the most favorable circumstances, five would differ among each other by an amount equal to the quantities upon which this "natural law" is supported, we assert what every competent analyst knows to be true, and what moreover pronounces most emphatically upon the value of such investigations.

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[*] Misprinted *twenty*-one, on p. 31, 4th Kentucky Report.

[†] Misprinted on p. 207, 3d Ky. Rep., where the difference is made 0·045 instead of 0·078, as given above from the tabulated analyses.

It is therefore our conclusion, that while, as has long been known, the soil loses in mineral matter what the crop gains, it is doubtful if in any given case chemical analysis can indicate this difference with certainty, for the reasons that the accidents which affect analysis make the limits of inaccuracy, to cover more than the loss by years of cropping. When we take into account the changes that are constantly progressing in the soil when under cultivation—changes by which the disintegration is hastened, changes by which it is made in many instances more retentive of soluble matters—when we remember that most cultivated crops, although they carry off in seed, stem and foliage a quantity of mineral matters, yet derive these in part from a depth below the range of analysis, and in their roots or stubble, leave upon the surface, salts brought up from a considerable depth—we perceive that the problem is so complicated with compensations and variable quantities as to put it beyond the reach of quantitative chemical analysis.

If, in any case, soil-analysis does show or appear to show the exhaustion of the soil, it is however, the appeal to experience which *proves* it, and as this is the first, most obvious, and an entirely sufficient proof, we do not see the value of the "law" that has 10 per cent (eight-seventy-ninths) of exceptions, the existence of which like that of the rule itself, is only to be established by comparison with the plain agricultural fact.

In short, if we admit the result as Drs. Owen and Peter would have it—of what use or interest is it?

The 3d point, is that analysis shows "the peculiarities of the soils derived from different geological formations." Says Dr. Owen, "these analyses most distinctly show that certain geological formations impart to the soil more of the important mineral fertilizers than others." The reader will be able "to see that it is those formations which are composed of easily disintegrating materials, which, all other things being equal, yield the soils richest in phosphoric acid, lime and potash; and at the same time contain the quantity of alumina and oxyd of iron necessary to render them sufficiently retentive and attractive of atmospheric water and ammonia; therefore these soils are the best adapted for those grains and crops which require the largest proportion of these ingredients." "He will moreover be able to trace the gradual diminution in the proportion of the more important mineral ingredients, down from these extraordinarily fertile soils derived from the highly fossiliferous, argillo-calcareous beds of the lower Silurian, the Cretaceous and the Tertiary systems of the West; through the silico-calcareous soils of the upper Silurian, Devonian and Sub-Carboniferous limestone strata, in which fossils are either more sparingly distributed or, in some cases almost wanting, and which are far less easy of decomposition; thence through the argillo-silicious soils of the Coal measures with only locally organic remains, and these chiefly of plants, down to the more purely silicious soils prevalent where the non-fossiliferous sandstones of the Coal measures and of the Millstone Grit, prevail to the exclusion of either shales or limestones and which afford the most unproductive soils as yet analyzed." While it is to be expected that rocks of complex origin rich in organic remains—which are evidences that the rocks themselves originally resulted from the deposition of the washings of fertile lands—should yield richer soils than sandstones or limestones, we do not see that analysis of the soil makes the fact more evident Knowledge of the composition of a rock enables us to judge in a general way of the value of the soil, so far as this depends upon chemical characters. We do not see what is gained by further analyses of the soil. It would appear that the cheap mental processes of deduction or inference may accomplish here in a moment all that an expensive analysis can show.

We fail moreover to perceive that analysis shows "the *peculiarities* of the soils derived from the different geological formations." In a cretaceous or limestone soil we of course expect to find much carbonate of lime, and in a sandstone or millstone grit soil much insoluble silica or silicates, but the quantities of phosphoric acid, potash and sulphuric acid do not appear to bear any definite relation to their geological origin. It is impossible to represent the composition of the soil of any geological formation by a typical statement of percentages, or to point out its peculiarities further than by an undefinable more or less. Although Kentucky and Arkansas lie mostly or altogether beyond the influence of drift, yet the action of running water in its constant passage from hill-top to valley has to a great degree obliterated from the soils those *peculiar* differences to be found among the rocks from which they have been derived.

A careful examination of the analyses recorded in the Arkansas survey shows that the average composition of the eight soils analyzed from the Lower Silurian and of the fourteen from the millstone grit, compare as follows, in regard to the more important ingredients:

		Carb. lime.	Magnesia.	Phosphoric acid.	Sulphuric acid.	Potash.
Lower Silurian, average of	8 soils,	0·533	0·485	0·184	0·052	0·355
Millstone grit, "	14 "	0·215	0·531	0·180	0·057	0·148

Here we see that the soils of the poorest formation are inferior to those of the richest only in carbonate of lime and potash. Of the soils of the millstone grit, nine are richer in carb. lime than the poorest of the Silurian, and five of the former contain more potash than the poorest of the latter. On the other hand but two of the Silurian soils have higher percentages of either carb.

lime or potash, than the richest soil of the millstone grit. If these figures demonstrate anything, it is the fact, that no geological formation has the absolute monopoly of either barren or fertile soils. If the analyses of Dr. Peter show the "peculiarities" of the soils of any geological age, then certainly these peculiarities are not remarkably peculiar!

On page 50 of the 2d Ark. Rep., Dr. Owen remarks as follows: "With the table of the composition of the ashes of plants to refer to, appended to this Report, and after becoming acquainted with the usual proportions of mineral constituents in an average soil, information which is easily acquired by looking over the table of soil analyses in this Report, it is easy for any individual to see, when he is provided with a reliable analysis of his soil, not only to what crop it is best adapted, but what kind of mineral fertilizers, if any, it requires as a manure, and how it compares in fertility to the various grades of soils from other farms and other states. Is not this knowledge of some value to the farmer?"

The above, we are of opinion, proceeded rather from the generous heart than from the critical brain of its lamented author. Had he attempted to *do* the things which he believed to be so easy, we are sure his statements would have lost somewhat of their directness and would have appeared in a form highly modified from the above. "The usual proportion of ingredients in an average soil." What is an average soil? Our only way of deciding what is such a soil consists in noting the average yield of soils. But the yield depends not alone on the soil, but upon climate, weather, tillage and various incidents and accidents. It depends not on the composition of the soil—not on the "proportion of ingredients" alone, but likewise on the condition of those ingredients, their state of combination, their solubility. It depends also on the physical characters of the soil, which determine the relations of the crop to the essential conditions of regulated heat and moisture. The soil is not less important to the plant in its function of home than in its function of food, the lodgings are of equal influence with the board. It is a nice work to balance these varying circumstances, many of which have as yet in our science, no shadow of a numerical expression, and then to say how many thousandths of a per cent of potash, lime, phosphoric acid, &c., belong to the "average soil."

Dr. Peter has indeed attempted to show the degree of availability of the elements of the soil by the following process. "A quantity, generally thirty grammes of the air-dried soil is placed in an eight-ounce strong vial, with a close fitting stopper, and the bottle is filled up with distilled water which has been charged with pure carbonic acid gas, under a pressure of about two atmospheres. The bottle is allowed to remain for about a month at a temperature about that of summer heat." The matters thus dissolved were then analyzed as usual. These results have this value, they show that the water of the soil is capable of dissolving all the elements of the food of plants. They furnish moreover a *rough* comparative view of the available matters in different soils. Beyond this we cannot attach any value to them.

We now come to Dr. Owen's 4th result of soil-analyses, embodied in the above quotation, and repeated on p. 30 of the 4th Ky. Rep., viz: its power of indicating "the suitability of the soil for any particular crop." Closely related to this is the 5th item, viz., that analysis can show "what addition any soil, either uncultivated or cultivated, requires to render it productive and remunerative for any given crop; and, of course, the deficiency in the soil of one or more of the eleven elements determined by chemical analysis."

We cannot help feeling that the above assertions which are here made unqualifiedly, were intended to be understood with a large amount of reserve and subject to various conditions. Otherwise we must regard them quite unjustified, if not absurd. The chemical analysis of soil reveals nothing as to its tenacity or lightness, its porosity or retentiveness for water, yet these physical and mechanical conditions more than anything else determine the adaptation of a soil for any particular crop. The best grass lands are not the best wheat lands—and although it would scarcely be questioned that wheat requires a richer soil than grass in order to produce an average crop, and although as we know, it often happens that many successive hay crops may be removed from a meadow without sensible diminution of the yield, while uninterrupted cropping with wheat nearly always reduces the capacity of the soil in a very few years below a profitable point; yet each average hay crop removes from a field more of every ingredient of vegetation than the grain and straw together of an average harvest of wheat.

Such at least is the testimony borne by the most recent and trustworthy data. Dr. Anderson of Glasgow basing his calculations on the best analyses and on the extensive agricultural statistics gathered in late years by the Highland and Ag. Society of Scotland, makes the following estimate of the amount of the principal ingredients removed from an acre by average crops of seven staple British farm products. See table on next page.—*Trans. Highland and Ag. Soc.*, 1861, p. 568.

On comparing the amount of matters removed from an acre by the wheat and hay crops, we find that the latter requires four times as much potash, lime and sulphuric acid; twice as much silica and one-fifth more nitrogen.

Again we know that oats are raised on soils which are considered too poor for the profitable production of wheat, and the

	Produce per Imperial Acre.	Total Weight in lbs.	Total Mineral Matters.	Potash.	Soda.	Lime.	Magnesia.	Chlorine.	Sulphuric acid.	Phosphoric acid.	Silica.	Nitrogen.
Wheat—												
Grain, ...	28 bush. at 60 lb.	1680	34·12	10·11	1·20	1·04	4·80	....	0·32	16·22	0·43	29·20
Straw, ...	1 ton, 3 cwt.	2576	114·48	20·70	2·84	8·53	2·23	....	3·55	3·16	73·47	16·13
Total, ...		....	148·60	30·81	4·04	9·57	7·03	....	3·87	19·38	73·90	45·33
Barley—												
Grain, ...	33 bush. at 53 lb.	1749	44·24	9·40	0·30	0·76	3·10	1·12	0·85	15·52	13·19	34·98
Straw, ...	18 cwt.	2016	99·14	11·24	1·14	5·81	2·75	1·30	1·10	7·22	68·58	6·03
Total, ...		....	143·38	20·64	1·44	6·57	5·85	2·42	1·95	22·74	81·77	41·01
Oats—												
Grain, ...	34 bush. at 40 lb.	1360	48·89	11·00	....	5·31	4·04	0·20	....	26·07	2·27	27·54
Straw, ...	1 ton.	2240	143·53	30·71	6·10	10·29	5·50	5·55	5·18	7·35	72·85	14·10
Total, ...		....	192·42	41·71	6·10	15·60	9·54	5·75	5·18	33·42	75·12	41·64
Beans and Peas—												
Grain, ...	25 bush. at 60 lb.	1650	55·97	30·00	0·31	3·01	4·00	....	1·76	16·65	0·24	46·10
Straw, ...	1 ton.	2240	108·51	48·61	13·14	29·37	3·74	7·00	2·07	0·74	3·84	26·88
Total, ...		....	164·48	78·61	13·45	32·38	7·74	7·00	3·83	17·39	4·08	72·98
Turnips, ...	13½ tons.	30,240	213·75	57·35	44·71	28·60	4·65	10·35	39·02	22·57	6·50	60·48
Potatoes,.......	3 tons.	6720	55·58	28·92	2·85	1·20	2·11	3·21	10·24	5·76	1·29	26·00
Hay, ...	2½ tons.	5600	391·31	129·79	4·80	35·46	9·62	39·61	16·57	21·79	133·67	56·22

table shows us that an average crop of oats requires more of every mineral ingredient than is needful for a corresponding wheat crop.

In fact, wheat is the crop, to grow which continuously requires, according to universal agricultural experience, land richer than that needed for any other of the seven crops whose chemical statistics are given in Dr. Anderson's Table, and notwithstanding, with exception of barley and the potato-tuber, it removes the least from the soil.

The farmer knows that wheat delights in a deep, rather heavy soil, one which holds moisture well, and yet is not wet. Barley and oats flourish on soils that are too dry and light, and grass on those which are too wet for wheat.

But how does the matter stand when these external conditions are taken into account? Does not analysis aid us then in a good degree? Let us take a case similar to what has repeatedly occurred in actual practice. We have a soil which as the result of long cultivation or from natural deficiencies, is incapable of yielding a remunerative crop of wheat. Its texture is good, it has produced wheat abundantly, and needs nothing but a little of the right kind of manure to restore its power of giving a crop. We put upon it Peruvian guano at the rate of 300 lbs. per acre, and the harvest is a good one. The entire addition to the soil is but $\frac{300}{3000000}$ths = one hundredth per cent. The amounts of phosphoric acid, of alkaline earths and nitrogen added, are for each, but one six-hundredth per cent of the soil, taken to the depth of a foot. These quantities are rather minute for even the improved analysis of the present time to estimate successfully.

Calculations like this show that the chemist cannot discriminate by his analysis between; 1st, a soil which is unproductive from the temporary exhaustion of some of its available ingredients; 2d, the same soil which is rendered fertile again for a year by the use of 300 lbs. of guano; and 3d, the same, made over-rich so that nothing will grow on it, by an application of a ton of guano.

On page 18 of the 2d Ky. Rep., Dr. Owen remarks as follows: "During last summer a soil was collected in Bullit county, from an old field which had been fifty or sixty years in cultivation, and which will now no longer produce clover. I venture to predict that when the analysis of this soil shall be completed it will be found to be deficient in some of these constituents,* and the analysis will probably show what other green crop might succeed better for the renovation of such land."

On page 230 of the 3d Ky. Rep., Dr. Peter gives the analysis of this soil, and says, "The inability of this soil to produce clover is explained by its very small proportion of lime, and rather small amount of sulphuric and phosphoric acids. The addition of plaster of Paris or some of the calcareous marls would probably restore it to the capability of supporting a clover crop."

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* The mineral ingredients of plants.

The percentages of the ingredients which Dr. Peter considers deficient, are as follows:

Carbonate of lime, - - -	0·072 =lime 0·040
Sulphuric acid, - - -	0·055
Phosphoric acid, - - -	0·070

Small as are these quantities, the smallest of them, viz., that of lime, yet amounts to 1200 lbs. per acre, which is enough to supply 10 clover crops of 3 tons each, and as by the analysis it all exists in the form of carbonate, it must all be available. We know from the vegetation experiments of Boussingault, Ville, and Sachs, that plants are capable of absorbing from a limited amount of soil *the whole* of any soluble nutritive substance present, provided its quantity be no more than the plants require, and the other elements of fertility are at hand in excess.

Twelve or thirteen years ago, Dr. Anderson in his capacity of Chemist to the Highland and Ag. Society of Scotland, had occasion to investigate two soils which had become "clover-sick," and he caused them, together with similar adjacent soils which still produced clover, to be most minutely analyzed. Without reproducing his figures, which may be found in the Trans. of the Highland and Ag. Soc. for 1849–51, p. 204, we will merely quote some of the remarks which accompany the analyses: "The results of these analyses are certainly of an unexpected character, and appear to me to indicate that, in this instance the failure of the clover cannot have been dependent upon the chemical constitution of the soil. In both cases the results of the analyses of each pair do not present a greater difference than would be obtained from the analyses of two portions of soil from different parts of any field."

In the present year, Stoeckhardt (*Chemischer Ackersmann*, No. 2, 1861, p. 85), has published an account of several "clover-sick" soils from Schlanstaedt, which reveal to analysis a greater content of *every nutritive mineral ingredient* both soluble in water and in acids, than exists in another soil from Frankenstein which produces clover and wheat as well. What proves beyond a doubt that the inability of these soils to yield clover depends upon something besides their chemical constitution, is the fact that lucerne and esparsette still flourish upon them admirably, and further, clover itself, if sown with one of these last mentioned crops, succeeds very well.

A great truth in agriculture is this: Each kind of agricultural plant requires that its seeds be surrounded with certain conditions in order that they may germinate readily and healthfully, so that when the mother cotyledons are exhausted, the young plants shall attack the stores of food in the soil with that vigor which is needful in order to appropriate them without hindrance.

The fact that winter wheat is more delicate and fastidious in its infancy than most other crops, is perhaps the main reason why it does not succeed well on many good lands, and why it cannot be continuously produced from the same soil, year after year. It is a matter of experience that wheat requires a rather firm seed-bed: beans, oats and mangold-wurzel approach wheat in their requirements, while barley, peas and turnips are best suited in a light tilth. On the other hand, climate, weather and tillage so influence the character of the soil, that even on light lands, wheat may find all the conditions of its growth. The bed which is produced by inverting a clover sod, and allowing it to consolidate by time and rains, or by passing a heavy roller over it, is eminently adapted to wheat, even on a rather light soil.

The fact that in the cases given above from Stoeckhardt, clover succeeded when sown with lucerne or esparsette, would indicate that, possibly, the condition of the seed-bed was the cause of failure.

These and other facts which might be adduced to almost any extent, indicate sufficiently that chemical analysis alone, even if we admit its full nicety and accuracy, can at the best furnish us with a knowledge of but a few of many conditions which must coöperate in profitable agricultural production, and as a consequence, its part in guiding the farmer is but very subordinate. Taking into the account its evident uncertainty and clumsiness when applied to estimating the minute quantities which affect vegetable growth, the part it can play becomes still more subordinate—we hesitate not to say, insignificant.

As we write, a fragment from a Scientific Journal brings to our notice a discovery which if real, strengthens our views in an unexpected manner. It is well known that iodine is so immensely diluted in sea-water—the soil of marine-plants—that none of our tests though they are among the most delicate, serve to detect it directly, and it is doubtful if it has been detected even in the highly concentrated mother liquors which remain after separating the crystallizable salts, yet the fuci find and accumulate it, and we must grant that it is present there for them, in sufficient quantity.

Again, Prince Salm Horstmar several years since, in his admirable researches on the influence of the individual mineral ingredients of plants on the development of oats and barley, found that he could not by any possibility exclude chlorine from his experimental plants. His soils and pots, the salts and water he fed his plants with were so purified that he could not detect this element in them, and yet he invariably discovered it in the ashes of the plants. So too he found titanic acid in the produce grown on the most carefully purified soils. Now, it is mentioned in the "Chemical News" that he finds *a few hundredths of lithium are indispensable* to the ripening of barley. This element Bunsen has but recently shown to be everywhere distributed, yet it has been hitherto entirely unnoticed in all soil- and plant-analyses because of its occurrence in almost infinitesimal quantity.

It must be well borne in mind that Agriculture herself—so-called Practice—is able of her own resources to judge somewhat of the value of soils, is able to know if a soil be fertile or poor, is able to pronounce upon its adaptation to crops, and can to a certain extent decide what is a good manure for this or that field.

We are free to assert that the knowledge which is now to be gathered from experience, is able in ninety-nine cases out of one hundred, to give a more truthful verdict as to the capacity of a soil, that any amount of analysis, chemical, mechanical or otherwise, can do. We would give more for the opinion of an old intelligent farmer than for that of the most skilled chemist in most questions connected with farming. Doubtless the farmer would make some blunders from which chemistry might save him, but the chemist would be likely to do more violence to agriculture, than the farmer would to chemistry.

By these statements which may, but should not surprise some of our scientific friends, we merely intend to express an opinion as to the present relative position towards agriculture of those who regard the art from a chemical, and those who see it from an experimental point of view.

If any one has fuller and more inspiring notions of the importance of science in its applications to agriculture than we have, we desire to sit at his feet and share the higher *afflatus*. But our inspiration, if it be of the sort that works enduring benefit, must be based on clear ideas of the directions in which advance is possible and on a full perception of the difficulties that lie before us, and the means of overcoming them.

We have great faith that chemistry and that chemical analysis have done and are to do a work for agriculture, that shall lay that venerable art under everlasting obligations to the youthful science. But not by soil-analyses alone or mainly is this to be achieved. We do not assert that soil-analysis is worthless—we believe that the probabilities of its uselessness in direct application to practice are so great that we would rarely base any operations on it alone, and yet it may in many cases, promote science and give us data for conclusions that are of practical use. But for these purposes it must form part of a system of observations and trials, must be a step in some research, must stand not as the index to a barren fact, but as the revelator of fruitful ideas.

We hold that soil-analysis long ago played out the part which Dr. Peter would have it perform. In the hands of Sprengel it was fertile with new truth, but it must henceforth be a tool for occasional use, and not an engine of discovery. With our advance in knowledge there must be an advance in methods of finding out the unknown. Soil-analysis was indeed a means of insight into the secrets of vegetable growth, but it carried with it the measure of its limit. What we call telescopes do not enable us to see *the end!*

To study the soil in the hope of benefitting agriculture, we must regard all its relations to the plant. We must examine it not merely from those points of view which theoretical chemistry suggests, but especially from those which a knowledge of practical agriculture furnishes. This is becoming more and more the habit of agricultural chemists and the results are of the happiest kind.

Let us remember what the illustrious Nestor of Agricultural Science, Boussingault, has said as the summing up of his protracted experience and study.

"At an epoch not far distant it was believed that a strict connexion existed between the composition and the quality of arable soil. Numerous analyses shortly modified this opinion as too positive. The sagacious Schübler even sought to prove in a research that has become classic, that the fertility of a soil depends more upon its physical properties, its state of aggregation, power of absorption, &c., than upon its chemical constitution."

"The physical properties, in my opinion, do not enable us, more than the chemical composition, to pronounce upon the degree of fertility of the soil. To decide this point with some measure of certainty, it is indispensable to have recourse to direct observation; it is necessary to cultivate a plant in the soil, and ascertain with what vigor it developes there: the analysis of the plant afterward intervenes usefully, to indicate the kind and quantity of the elements that have been assimilated."—("*De la Terre Végétale considerée dans ses Effets sur la Vegetation,*" page 283 of "*Agronomie, Chimie agricole et Physiologie, Tome premier,* 1860.")

There has been much progress made in our knowledge of the soil during the last ten years. This advance has not consisted in revealing to us the presence of new elements (lithia perhaps excepted), nor in fixing with any more certainty the quantitative limits which separate barrenness from fertility, it has not shown what is the composition of a Silurian or a Sub-Carboniferous, a Drift or a Tertiary soil, it has not defined the soil adapted to wheat or that productive of clover, it has not indicated the manures which this or that soil needs; but content with the fact that all soils which naturally support vegetation contain the elements of vegetation, it has sought to ascertain in what forms these elements are assimilable, how they may be made available, what changes or reactions in the soil affect its productiveness; how fertilizers act indirectly (their influence often having no relation to any supposable direct action), how the soil affects the life of the plant otherwise than by feeding it, &c. &c.

We are approaching in fact by slow degrees to an understanding of the physiological significance of the soil, a grand result to which chemistry and physics coöperate.

We trust that in the future, the American people will not less but more appreciate the value of science in its practical and especially its agricultural bearings; that here, as in Germany, France and England, the labors of those who seek to unite Practice with Science may be fostered and sustained. But to this end scientific men must be cautious that in endeavoring to help, however honestly and laboriously they may work, they do not hinder.

Sheffield Scientific School, August 20th, 1861.

ART. XXIII.—*The Great Comet of* 1861.*

1. *Observations at New Haven.*

ON Sunday evening June 30th between 8 and 9 o'clock, there was observed at New Haven, in the northern part of the heavens in an opening between the clouds, and at an elevation of about ten degrees, a nebulous body of unusual brilliancy. Its appearance was similar to that of the planet Jupiter shining through a thin mist; and it was nearly as conspicuous an object in the heavens as Jupiter, although this was due not wholly to the intensity of its light, but partly to its extent of surface, its apparent diameter being about equal to that of the full moon. It was at once suspected that this body was a comet; but this conclusion was adopted with some reserve, on account of the unusual brilliancy and sudden apparition of the meteor. This light was soon concealed by a cloud; but about half an hour later, a larger opening in the clouds disclosed the tail of the comet, in the form of a bright streamer, with sides nearly straight and parallel, and pretty sharply defined. The head of the comet was now invisible; but a little later, both head and tail were seen simultaneously, forming together one of the most brilliant comets of the last fifty years, and astonishing every one by the suddenness of its development. Mr. R. W. Wright of this city marked the position of the comet's head upon a star chart as accurately as he was able, and hence concludes that about a quarter before nine o'clock, June 30, its R. A. was 108° and Dec. 47° N.

On Monday it was ascertained that on Saturday evening, several individuals had noticed in the north a bright streamer rising to a great height above the horizon, and it was at once concluded that this was the tail of the same comet. The daily newspapers report that the head of the comet was seen on Saturday evening at Columbus, in Ohio, but it is not known that any one made any accurate determination of its place.

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* **The first part of this paper was published in advance of this issue, July 20th. We now reproduce it with important additions.—EDS.**

On Monday night at New Haven the sky was overcast; but on Tuesday evening July 2d, the sky was mostly clear, and the comet very conspicuous; although it was thought that its head was not as brilliant as it had been on Sunday evening. At 9^h 31^m P. M., the position of the head was in R. A. 8^h 41^m, and Dec. 63° 5′ N. Seen through a telescope of five inches aperture, with a power of 55, the head was fully thirty minutes in diameter. Near the center of the nebulosity appeared a very brilliant nucleus, from which emanated a luminous sector, whose opening was about 90°, one side being nearly vertical and the other or right side was nearly horizontal. This brush of light extended two minutes from the nucleus. The tail of the comet could be traced to a distance of 90° from the head.

On Wednesday evening the sky was again clear, and the comet was observed to great advantage, but its brilliancy had palpably declined since Sunday evening. At 9^h 5^m P. M., its head was in R. A. 9^h 52^m and Dec. 66° 10′ N. Seen through the telescope, the coma had about the same extent as on the preceding evening, but the luminous sector already mentioned, had changed very noticeably. The sides of the sector were curved, the concavity being outwards, and the opening of the sector amounted to 136° when measured to the extremities of its arc, but the initial directions of the two sides formed an angle at the nucleus of about 90°. From the nucleus to the edge of the sector was 1′ 34″. Beyond this, there was a dark arch or band concentric with the nucleus, and beyond the dark band a luminous arch or envelope faint and misty, the middle line of which was 2′ 56″ from the nucleus. Beyond this there were faint indications of a second envelope, with an intervening dark arch, the whole forming a series of nearly concentric light and dark arches, similar to those observed in Donati's comet in 1858, and in Halley's comet in 1835. The tail of the comet on Wednesday evening could be traced through an arc of 95°, and the deviation of its axis from the position of direct opposition to the sun was about 12°, and toward the east, the axis produced cutting the ecliptic about 8° behind the sun's place.

The tail of the comet was carefully observed on several clear evenings, but the observations were more detailed and complete on the evening of July 3d. The northern edge grazed the star Lambda Draconis, passed about 15′ to the south of Kappa Draconis, and continued on through Iota Draconis, and far beyond it, in an arc of a great circle. The southern edge passed just to the north of H. 32, Ursae Majoris, grazed H. 30, Ursae Majoris, and continued on through the stars 3 and 8 Draconis. (According to the B. A. Catalogue these stars are Nos. 3496, 3358, 3968, and 4347.)

It broke off, or suddenly became faint, before it reached the distance of Alpha Draconis, at about 20° from the nucleus. From that point the tail continued as a much fainter milky band, decreasing very gradually in luminosity, and varying but little in apparent breadth. This breadth was less than one-half the breadth of the extremity of the brighter portion, which was about 3°. The southern edge of the narrower and fainter stream passed through Alpha Draconis, and grazed the stars Tau, Sigma, and Eta Herculis. The decreasing light of this stream vanished in the immediate vicinity of the Milky Way, to the east of β Ophiuchi. The extreme length of the tail was about 95°. The train of the comet was apparently made up of two distinct streams of luminous matter, differing greatly in width and length. The northern edges of the two were in the same line, but the extreme breadth of the shorter stream was much greater than that of the other. Its southern edge was badly defined, and somewhat concave outward. A very faint diffused light, rapidly widening out, could be traced far beyond the point where the sudden falling off of brightness occurred. This diffused light extended, on the evenings of July 4th and 5th, to the vicinity of Corona Borealis, or more than 40° from the nucleus, and attained to a width of 12° or 15°. Its southern edge passed just to the north of the star Theta Bootis. The breadth of the tail, as distinctly seen, at its broadest part, was about 3°. On the evening of June 30th, the estimated breadth was 5°; but a faint light on the south side was traced 5° farther, giving an extreme breadth of 10°. On July 4th, the tail was visibly forked about 2° below the star Alpha Draconis, or 15½° from the nucleus. On the following evening the point of forking was 3° or 4° above the same star. The nucleus had advanced 5½° toward it in the interval.

It was also observed, on the evening of July 4th, that by examining carefully it could be discerned that the long narrow stream increased in breadth about in proportion to the distance from the nucleus. At the point where first seen as a distinct stream its breadth was about 1½°.

Since July 5th the tail of the comet has decreased, from night to night, in brightness, as well as in length and breadth.

2. *Observations at Washington.*

The following places of the comet as observed at the U. S. Naval Observatory, Washington, have been communicated by Lieut. J. M. Gilliss, Superintendent of the Observatory. The observations were made by Mr. Ferguson with the large equatorial.

	M. T. Washington.	No. of comp.	α	δ
1861, July 2,	$9^h\ 55^m\ 19^s{\cdot}3$	10	$8^h\ 43^m\ 6^s{\cdot}97$	$+63^\circ\ 12'\ 14''{\cdot}7$
3	8 46 46·3	2	9 51 41·15	66 9 52·5
3	9 10 7·3	5	9 52 52·55	66 11 6·0
4	8 51 20·5	12	10 58 36·26	66 54 20·6
6	9 0 52·5	4	12 31 12·57	64 51 7·6

On the night of the 3d, it was observed on the meridian with the transit instrument by Prof. Robinson, U. S. N. and with the mural circle by Prof. Hubbard.

M. T. Washington.	α	δ
July 3 $15^h\ 21^m\ 46^s{\cdot}7$	$10^h\ 11^m\ 7^s.25$	$66^\circ\ 33'\ 31''{\cdot}5$

From the observations of the 2d, 4th and 6th, the following elements of its orbit were computed by Prof. Hubbard.

Perihelion passage 1861. June 11.43955. Washington mean time.

Longitude of the perihelion,	$249^\circ\ 11'\ 28''{\cdot}4$	Mean eqx. 1861·0
Longitude of ascending node,	278 58 32 ·1	
Inclination of the orbit,	85 37 35 ·5	
Perihelion distance,	0·821531	
	Motion direct.	

These elements give for the middle observation

$$\Delta l = -15''{\cdot}7 \ (c - o)$$
$$\Delta b = -10\ {\cdot}6$$

It is obvious from these elements, that this comet is not the same as the comet of 1556 (called Charles Vth's comet) whose return has been anticipated for several years; nor do these elements bear any resemblance to those of any comet in the published catalogues. We must conclude then that this comet is a new one, whose orbit has never before been computed.*

3. *The Comet, as seen at the Observatory of Harvard College, Cambridge, Mass.* Communicated by G. P. Bond, Director.

THE comet was first seen at the Observatory of Harvard College in the early twilight on the evening of Tuesday, July 2d. The sky was clouded on the first and on the thirtieth. On Saturday the twenty-ninth of June the air was hazy preventing the usual sweeping for comets, although observations near the meridian were prosecuted until about 11^h P. M. Had the sky been clear, the tail of the comet would probably have been seen. A day or two previous, the western twilight had been explored with an opera glass, but at this time only the upper part of the tail could have been in sight, and it must have been too faint to attract notice.

The condition of the theory of cometary formation makes it very desirable that astronomers should devote more attention than they have hitherto been accustomed to do, to the accurate delineation of the curve of the tail among the stars. The present opportunity has been improved at the Observatory of Harvard College by making careful tracings of the boundaries of the rays through their entire extent upon star charts. The *Uranometria Nova* of Argelander was found to be especially convenient for the purpose, both from the exactness of the projection and the care taken in giving the proper magnitudes to the stars which greatly facilitates their identification. An uninterrupted series of clear nights from the second of July to the present time has very much favored us in preserving the continuity of the phenomena, which is a condition of the utmost importance for their future discussion.

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* We have received from Lt. Gilliss a paper on the physical characters of this comet, which will appear in November. Prof. Hubbard's latest computations however are given on p. 265.

The suddenness of the apparition of the comet in northern latitudes was one of the most impressive of its characteristics. On the 2d of July after the twilight had disappeared, the head, to the naked eye, was much brighter than a star of the first magnitude, if only the effective impression be taken into account, although as to intensity it was far inferior to α Lyræ, or even to α Ursæ Majoris. I should describe the head as nearly equal in brightness to that of the great comet of 1858 between the 30th of September and the 5th of October; it should be considered however that the present comet was better situated, from its higher position above the horizon at the end of twilight.

The aspect of the tail suggested a resemblance to the comet of March, 1843. It was a narrow, straight ray projected to a distance of one hundred and six degrees (106°) from the nucleus, being easily distinguishable quite up to the borders of the milky way. The boundaries for the most part were well defined and easily traced among the stars. It was not until after two or three hours of observation, that I could gain a clear comprehension of the structure of the tail or tails as they presented themselves to the naked eye and through a small opera glass. It was then evident that a diffuse, dim light with very uncertain outlines, apparently composed of hazy filaments, swept off in a strong curve towards the stars in the tail of Ursa Major—the southern edge directed as low as towards Mizar. This was evidently a broad curved tail, intersected on its curved side at the distance of a few degrees from the nucleus by the long straight ray which at the first glance, from its greatly superior brightness, seemed alone to constitute the tail. The two were in fact counterparts of the principal tail and the supplementary rays of the great comet of 1858, with this remarkable difference, that in the latter the straight rays were so far inferior in brightness to the curved tail as to have been recognized at only three observatories, those of Poulkova, Göttingen and Cambridge, U. S.—while with the present comet, the predominating feature was the straight ray to which the curved tail seemed scarcely more than a wisp-like appendage.

On further scrutiny with the aid of an opera-glass, two sharply cut and very narrow dark channels, bounding the principal ray, could be traced for ten or fifteen degrees from the nucleus; while outside of them, on either side, were two additional faint rays. The whole issue of nebulous matter from the nucleus far into the tail was curiously grooved and striated. It was noticed that both the principal ray and the dark channels penetrated within the outline of the curved tail, the latter being clearly separated from the principal ray even to the naked eye by a dark cleft just above their intersection. The well defined margin of the principal ray admitted of a very exact delineation, even as far as α Ophiuchi, 100° from its origin.

On the third, the bright rays and dark channels were traced to a distance of 40° from the nucleus, the principal ray to nearly 100°. Five or six alternations were distinguished, besides the hazy filaments constituting the curved tail. Some of the streaks could be traced quite up to the nucleus. The rays were not only separated by the dark channel parallel to their axes, but they were disconnected at intervals in the direction of their length.

On the fourth, there were two or more regions of contrary flexure on the north following margin of the ray, which, in a theoretical point of view, are of very great interest when taken in connection with the direction of the ray almost precisely in a great circle from the sun continued through the nucleus. This peculiarity presented itself still more decisively on the fifth, when the tortuous path of the ray could not be overlooked.

The very singular aspect of the northern edge of the principal ray for the first thirty or forty degrees of its course, attracted particular attention, and the charts were revised with all possible care. The sky was perfectly clear and the outlines so distinct that there could be no room for doubt as to the reality of the reflexure of the curve. Subsequently on projecting an arc of a great circle from the sun through the nucleus, it was found to lie clearly within the margin of the ray as far as a distance of thirty degrees (30°), from the nucleus, and there was still haziness beyond it almost to the distance of sixty degrees (60°). The charts on other dates indicate similar results, but the data cannot be properly discussed without requiring more labor than can be, at present, devoted to them.

Within the last few days the principal ray in the part near the nucleus, has assumed a more regular sweep in the direction opposed to that of the diffuse tail, which now reaches nearly to the centre of Corona Borealis, scarcely changing the course of its southern limit between κ and ι Bootis and ζ Coronæ Borealis from night to night.

The telescopic phenomena, though interesting, have not presented equally strongly defined features with those which characterized the great comet of 1858. We should perhaps except from this remark their structure for a day or two after their first emission from the nucleus. In this stage they were intersected by jets of luminous matter projected from the nucleus, and these limits were pretty clearly outlined.

On the 2d, portions of three were visible; the inner one showing a variety of details. In its outline and general aspect it was, like others which followed it, almost a fac-simile on an enlarged scale of some of those exhibited by the great comet of 1858. They rapidly faded, or were lost in the surrounding haze and their places were filled by new ones. Latterly, two, at most, could be seen at one time. It is quite important to remark that the successive envelopes resembled their predecessors not only in their general aspect but quite closely in the details of their structure; the luminous jets not issuing at random from all points alike of the nucleus, but continuing to follow a nearly similar course at each new discharge from its surface.

The most natural inference from this would seem to be that the nucleus, if it rotates at all upon an axis, does so very slowly. Of the pendulum-like vibrations of the luminous sectors ascribed by Bessel to the comet of Halley, nothing was seen; although the opportunity of witnessing them, had they existed, was very favorable, as the sectors were well displayed.

The nucleus was throughout brilliant, and, to appearance, solid, with a diameter of from 2″ to 3″.

The disposition of the nebulosity in the part of the tail contiguous to the head was nearly uniform throughout; the axial darkness being scarcely distinguishable, excepting on one occasion, July 3d.

The following positions have been derived from comparisons with neighboring stars.

1861.	Cambridge mean solar time.	α	δ
July 2,	$8^h\ 28^m\ 38^s$	$8^h\ 37^m\ 43^s.22$	+62° 51′ 17″·1
3,	8 21 33	9 49 15·85	66 6 15·3
3,	10 39 52	9 56 6·58	66 16 05·1
4,	10 39 18	11 2 7·48	66 53 26·4
5,	12 9 26	11 57 9·67	66 3 22·0
6,	9 17 39	12 31 2·60	64 51 33·3
8,	10 20 5	13 21 36·05	61 46 13·7
9,	10 40 47	13 37 37·88	60 21 45·2
10,	9 39 12	13 49 26·80	59 9 34·1
12,	11 57 47	14 8 0·59	56 54 47·2
13,	9 47 55	14 13 59·24	56 5 25·7

The nucleus admitted of very precise observations; indeed it is a curious fact that it would be quite possible by means of proper comparisons with neighboring stars, to obtain the differences of terrestrial longitudes of the principal points at which it was observed, with a degree of precision only surpassed by the more refined methods known in astronomy.

The near approach of the present comet to the earth and the sharply defined point of its nucleus, illustrates the practicability of a method of determining the solar parallax with perhaps greater exactness than can be attained by any other means. Many comets have stellar points for their nuclei, visible in the larger telescopes, which admit of as accurate comparisons with neighboring stars as is practicable in measurements among the stars themselves. Many such have appeared within the last fifteen years. Suppose such a comet to be suitably placed so as to be observed simultaneously in different quarters of the globe, when at a distance from the earth of less than one-twentieth of the sun's distance. Under favorable circumstances it would not be hazarding too much to say, that in the course of its apparition the probable error of the solar parallax could be reduced within smaller limits than is possible by means of transits of Venus or of any other method. Such an opportunity might possibly afford an improved value of the mass of the earth.

The following elements of the comet have been computed at the Observatory by Messrs. Safford and Hall.

Elements of Comet II, 1861. By T. H. SAFFORD.

$T =$ 1861, June 11.1878. Cambridge m. t.
log. $q =$ 9·91299
$\pi - ☊ = \omega$ 329° 40′·81
☊ $=$ 278 59·28
$i =$ 85 41·43
Motion direct.

By **A. HALL.**

$T =$ June 11.280. Washington m. t.
$\pi =$ 248° 51′ 50″
☊ $=$ 278 59 23
$i =$ 85 41 27
log. $q =$ 9·91352
Motion direct.

From the above, Mr. Safford finds the following approximate ephemeris for $10^h\ 24^m$ m. t. Washington, $\varDelta$ and r representing the distances from the earth and sun respectively.

1861.	α	δ	log. $\varDelta$	log. r
July 5,	178° 42′	+66° 8′	9·3404	9·9716
6,	188 21	64 45		
7,	195 24	63 13	9·4281	9·9800
8,	200 37	61 42		
9,	204 34	60 18	9·5051	9·9887
10,	207 39	59 2		
11,	210 5	57 54	9·5722	9·9975
12,	212 5	56 52		
13,	213 44	+55 57	9·6313	0·0066

The following, computed by Mr. Hall for Washington mean midnight, will give an idea of the path of the comet previous to its becoming visible in the northern hemisphere.

W. m. t.	α	δ	log. $\varDelta$
June 12·5,	61° 39′·1	−26° 32′·3	9·7222
16·5,	63 16 ·3	23 18 ·4	9·6182
20·5,	66 4 ·9	17 11 ·6	9·4831
24·5,	71 56 ·9	− 3 45 ·4	9·3105
28·5,	85 17 ·9	+27 41 ·5	9·1420
July 2·5,	132 15 ·9	63 34 ·0	9·2027
6·5,	188 53 ·9	64 38 ·6	9·3884
10·5,	207 50 ·1	+58 56 ·5	9·5420
June 29·0,	89 30 ·4	+33 8 ·4	9·1330

The comet passed its ascending node June 28·086, Washington m. t., when the difference between the heliocentric longitude of the comet and of the earth was 2° 0′. The difference between the geocentric longitude of the comet and of the sun was 12° 29′. Log. of distance of the comet from the earth =9·1529. Calling its brilliancy unity on July 2·5, we have

June 12·5,	Brilliancy =	0·11
16·5,		0·18
20·5,		0·33
24·5,		0·69
28·5,		1·41
July 2·5,		1·00
6·5,		0·50
10·5,		0·18

computed by the equation, brilliancy $= \frac{1}{r^2 \varDelta^2}$.

It will be seen that the comet, at about the time of its perihelion, must have been well situated for observation at the Cape of Good Hope and other points in southern latitudes. The *calculated* brilliancy is indeed much less than on the 2d of July, being only about one-tenth—but it is well known that the formula cannot be relied upon for the variations of the light of comets, which is greatly influenced by their positions relatively to their perihelia.

It is probable that at least the head of the comet was much brighter at the middle of June than it was after the first of July, and we shall wait with much interest for accounts of it from southern observatories, especially from the Cape of Good Hope, which has often, in similar emergencies, proved itself the most important astronomical position occupied by any existing observatory.

From the above elements, the diameter of the nucleus may be variously estimated at from one hundred and fifty to three or four hundred miles. On July 2d the breadth of the head at the nucleus was 156,000 miles, the height of the inner envelope 11,500 miles, and the length of the tail about 15,000,000 miles.

The comet was seen between one and two o'clock on Sunday morning, June 30th, by Dr. Brünnow, at the Observatory of Ann Arbor. This is the earliest authentic account of its visibility which has come to my notice. The head could not have been seen on Friday evening, although observations to that effect have been reported. The extremity of the tail, however, must have been within view for some time previous, though too faint to attract notice.

The reports current of the identity of the comet with those of 1264 and 1556 are without any foundation.

4. *Continuation of Account of Comet II,* 1861; *by* G. P. Bond, *Director of the Observatory of Harvard College.*

With the exception of a single cloudy night, observations were made at this Observatory, on every evening from the 2d to the 21st July inclusive. A like succession of clear skies rarely occurs here. At the latter date, the comet had become much fainter, and was besides obscured by the moonlight. In the region near the nucleus, the outlines of the envelope could no longer be discerned —the distribution of the brighter portions of the nebulosity, however, could be seen to resemble that of the earlier jets. This peculiarity has been preserved up to the beginning of August.

The tail during the last days of July, as seen through an opera-glass or comet-seeker, has continued to show the narrow ray, now reduced to about 12° in length.

Near the head it forms the right hand side of the tail (as presented in the evening), which is the brightest and best defined. Further on, it seems to bend to the right hand with a decided curve; at least this was the impression to the eye, but it has become a very faint object. The hazy tail curving to the left, maintains its position and general form, but is greatly diminished in extent and brightness. Mr. Safford has computed the following elements from observations:

June 30th,	Greenwich.
July 8th and 9th, July 18th.	Observatory of Harvard College.

Elements of Comet II, 1861.

$$\begin{array}{rll} T = & \text{June } 11.5018. & \text{Washington m. t.} \\ \log.\ q = & 9.915048 & \\ \pi - \Omega = & 330^\circ 20' 48''\cdot 1 & \left.\begin{array}{l} \\ \\ \\ \end{array}\right\} \text{M. Eq. } 1861.0 \\ \Omega = & 278\ 58\ 40\ \cdot 3 & \\ i = & 85\ 37\ \ 7\ \cdot 0 & \end{array}$$

The observations of June 30th and July 18th were exactly represented, and those of July 8th and 9th as follows, by the method of least squares. A comparison upon Aug. 3 is also added.

	c — o			c — o	
	$\Delta\alpha$	$\Delta\delta$		$\Delta\alpha$	$\Delta\delta$
July 8th,	+ 1″·7	0″·0	Aug. 3d,	− 8″·7	+ 23″·1
9th,	− 4 ·0	− 2 ·5			

The elements are somewhat closer approximations than those communicated in the previous notice, though not differing essentially from them.

The comet was observed extensively in Europe, on the evening of the 30th of June. The following passages are translated from a communication in No. 1316 of the *Astronomische Nachrichten*, from Prof. Secchi, Director of the Observatory of the Collegio Romano, dated Rome, July 1, 1861.

"Yesterday at 9^h 30^m, I noticed this immense train of light in the N.N.E. which I took for the smoke of the fireworks which they often have in the city on the festival of St. Peter and St. Paul, but I soon perceived that it was a comet. Its head was then so low that I could with difficulty take an approximate instrumental position, and at 9·39 m. t., I found its A. R. 6^h 37^m, and its dec. 45° 57′. After the head had set, the tail remained on the horizon, and was much more brilliant than the milky way.

About half an hour before midnight, the tail had reached its maximum development; it was very bright, and 6° broad at the pole star which was exactly in its centre. Thence it contracted considerably, the central part only remaining, which extended in a long band, tolerably bright, and about 2° broad, touching α Lyræ on one edge (the eastern), and passing the zenith, was lost in the milky way near the stars ζ and ε Aquilæ. Including the part below the horizon the length was 118°. This is truly surprising! After it had risen at 2 o'clock in the morning, I examined the details of the construction of the nucleus till about 3^h, and I found that the nucleus was elliptical, with its major axis situated transversely to the axis of the tail. The measure of this axis was 10″·05, and the minor axis was estimated as 2″ smaller, its light nearly equalling that of Saturn. It was well defined on all sides with a power of 200, only on the side towards the sun it was a little less sharp, while a great mass of jets of reddish light started from its edge in the form of a fan. The whole nucleus and the fan were surrounded with a halo, folding back in two branches to the right and left; the former longest and most brilliant; finally the whole was enveloped with a large, well defined, paraboloid segment. The distance from the apex of the paraboloid to the center of the nucleus was 3′ 12″, and that from the nucleus to the bend of the curved jets was 1′ 55″. The maximum breadth of the tail was about 6°." . . .

Dr. Pape of Altona has examined carefully the question whether the earth was at any time within the tail of the comet, and concludes that at its nearest approach we were about 2,400,000 miles distant from its nearest edge, on the morning of June 29th at $2^h\ 24^m$.

This result (so far as it relates to the principal ray) is also indicated by Mr. Hall's calculations communicated in the earlier notice. Mr. Hall states the difference of the heliocentric longitude of the comet and of the earth, at the passage of the node, June 28·086 Washington mean time, to be 2° 00′ and the log. distance of the comet from the earth 9·1529.

Dr. Pape finds for the nodal passage June 28·10 Washington mean time, difference of longitudes =1° 59′. The results in other particulars are also substantially the same.

The following remarks have been translated from Dr. Pape's article in No. 1316 of the *Astronomische Nachrichten*:

"From the position of the comet with respect to the sun and the earth, we might conjecture the latter to have passed through the tail of the comet on the 28th or 29th of last June. As such a collision is of some interest, I have undertaken a slight calculation to obtain closer details. The passage of the comet through its ascending node took place, Berlin time, June 28·35 (28·10 Washington).

At this time, its true anomaly was 29° 37′·5, log. $r=9·94449$, its heliocentric longitude 278° 59′, its distance from the earth's orbit =0·1161. At the same time the heliocentric longitude of the earth was 277° 0′. Assuming that the axis of the tail coincided with the prolongation of the radius vector, the earth was at a distance of 0·0352 from this axis. (Mean distance of the earth from the sun =1.)

A consideration of the dimensions of the tail shows that, according to the data, no collision took place.

That point of the tail which lay in the earth's orbit was at a distance of =0·1162 from the nucleus; to determine the diameter of the tail at that point, I subjoin an observation of the 3d of July.

At 10 o'clock on the evening of that day, the angle which the prolongation of the radius vector of the comet formed with the direction from the earth to the comet, was =57° 40′; a point in the tail (which could be traced for about 60 degrees) at a distance =0·1162, had an apparent distance of 42° 24′ from the nucleus, and at the same time that the distance of this point from the earth =0·1457.

At a distance of about 42° from the nucleus, the breadth of the tail may have been 3°; hence it follows that its true diameter was 0·0076.

Now on June 28·35, the distance of the earth from the axis of the tail was 0·0352, so that the nearest particles of the tail passed by the earth at a distance of 0·031. This result however rests upon the supposition that the tail lay in the prolongation of the radius vector. In fact, it appears to have been curved in the plane of the orbit, so that the remotest particles of the tail passed through the plane of the ecliptic earlier than the nucleus. But in no case, did any collision take place. According to the above hypothesis, the least distance of the earth from the nearest of the tail particles was 0·025 at about 12 hours after the passage of the comet through the node."

Altona, July 9th, 1861.

The above calculations of Pape relate to the straight bright ray. There was, however, as will be seen by reference to the preceding part of this notice, a great mass of diffuse light which separated from the ray, and swept off towards *Corona Borealis* in the early part of July, reaching on the 4th to a distance of 40° from the nucleus, and having a breadth of 12° or 15°. This would have barely grazed the earth on the 30th of June. In a late number of the Bulletin of the Imperial Observatory at Paris, Mr. Hind suggests that a peculiar illumination of the sky noticed in England on that date was possibly "attributable to the commingling of the matter forming the tail of the comet with the earth's atmosphere." The observations on this day have a peculiar interest, from the fact that, at about 10^h mean time Greenwich, the earth passed the plane of the comet's orbit, and the outline of the tail presented to us was that of the section formed by a plane perpendicular to its orbit.

From its brilliancy and nearness to the earth, the opportunity of defining this section was perhaps the best which the history of astronomy has afforded. It is to be hoped that European astronomers, who it seems had the advantage of clear skies and a favorable position of the comet at the very hour of our crossing the orbit, have made good use of the occasion by recording carefully all peculiarities of the physical aspect.

Prof. Secchi has defined the position of the tail at $11^h\ 30^m$ m. t. at Rome, June 30, about 55^m after the earth's passage through the orbit plane. Comparing the position of an arc of a great circle drawn from the sun through the nucleus, which must have corresponded almost precisely with the projection of the plane of the orbit, we find that at the pole star the centre of the ray was about $1\frac{1}{3}°$ to the *east* of the circle.

At α Lyræ the eastern edge was at the same distance to the west of the circle, so that the entire breadth was on that side; the centre being distant

$$1\tfrac{1}{3}° + \tfrac{1}{2}\ (\text{breadth of tail}).$$

Again at ζ and ε Aquilæ the tail was about 4° to the east of the circle. There was therefore a decided *bend* in the tail which on account of the direction of the time of vision must have been *out of the plane of the orbit.* That its entire breadth should have been out of this plane, as it seems to have been near α Lyræ, is indeed a most remarkable feature and very important in its bearing upon the theory of the forces concerned in producing it. At later dates, as we have remarked in a previous notice, the tortuous path of the ray was obvious.

Cambridge, Mass., Aug. 3, 1861.

Cambridge, August 8th.

P. S.—A letter of the date June 17, received from Dr. Moesta of the National Observatory, Santiago, Chili, announces the appearance of "a brilliant comet" early in the month. Its position and motion agree with the ephemeris communicated in a former part of this notice. We extract the following observations:

	Mean time Santiago.	α	δ
1861, June 10,	$18^h\ 18^m\ 26^s\cdot 1$	$4^h\ 3^m\ 52^s\cdot 92$	$-27°\ 51'\ 8''\cdot 8$
12,	17 46 16 ·4	4 6 5 ·17	−26 47 14 ·7

Cambridge, August 10th.

Mr. Safford's latest elements represent the Santiago observations as follows:—

	comp.—obs.	
	$\Delta\alpha$	$\Delta\delta$
1861, June 10,	$-36''\cdot 0$	$+22''\cdot 9$
12,	$-15\cdot 7$	$-16\cdot 6$

There can not therefore be any considerable deviation of the orbit from a parabola.

The tail of the comet was seen 40° long by Liais at Rio Janeiro on the 14th of June.

5. *Additional Observations at the U. S. Naval Observatory, Washington.* Communicated by Lt. J. M. Gilliss, U. S. N.*

Upon attempting to compute an orbit for the comet, Prof. Hubbard found it impossible to represent the observed path by a parabola, and then obtained by the Gaussian method the following hyperbolic elements, based upon the places obtained by Mr. Ferguson on the 2d, 8th, and 17th of July. They give for the middle date the values $\Delta l=-0''\cdot 33$, $\Delta b=-0''\cdot 16$.

Time of perihelion passage,	1861, June 11.85294.	Wash. M. T.
Long. of perihelion,	$249°\ 44'\ 44''\cdot 58$	M. equinox, 1861·0
" " node,	278 59 49 ·72	
Inclination,	85 56 8 ·86	
Excentricity,	1·0265470	
Perihelion distance,	0·7453901	

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* From Lt. Gilliss' paper, to appear in full in our next issue.

The following is the list of observations, as far as reduced and compared with the above elements:

Wash. M. T.		α	c.—o.	δ	c.—o.
	d	° ′ ″	″	° ′ ″	″
July	2·41250	130 48 6·8	+ 2·4	63 12 48 5	+ 1·3
	3·36480	147 56 52·0	+17·9	66 10 2·4	− 0·6
	·38102	148 14 43·1	− 9·5	66 11 49·9	− 4·7
	·63908	152 46 48·8	+24·8	66 34 17·6	− 2·5
	4·36786	164 40 26·8	+18·6	66 54 21·2	− 5 5
	6·37421	187 49 0·3	+ 2·5	64 51 1·7	− 5·8
	8·36769	200 12 23·3	− 0·5	61 50 2·0	− 0·8
	10 53104	207 46 0·2	− 1·4	58 58 55·8	− 9·4
	11·45260	209 59 3·0	+ 2·3	57 56 58·2	−10·2
	12·42192	211 54 9·1	+ 8·9	56 58 12·1	− 7·8
	14·43062	214 56 37·2	+ 7·5	55 14 25·5	− 6·1
	16 37969	217 6 9·6	−13·4	53 52 5·4	+ 5·2
	17·35071	217 58 33·0	− 4·8	53 16 37·0	+ 1·5
	·42154	218 1 58·8	+ 4 2	53 14 13·7	− 3·8
	20·40982	220 9 54·0	+16·7	51 42 28 5	− 3·8
	23 37224	221 44 40·7	+19·5	50 30 17·2	− 9·1
	24 35193	222 11 27·8	+28·1	50 9 26·8	− 9·8
	25 36437	222 37 26·0	+32·5	49 49 15·3	−13·2
	27·37080	223 24 38·3	+36·0	49 12 25·8	− 8·9

From these residuals it is evident that the orbit requires yet some correction, not sufficient, however, it is believed, to change its decided hyperbolic nature.

6. *Elements of the Comet, in a letter to the Editors*, by Mr. M. C. STEVENS.

I took the position of the late great comet on the 4th, 8th and 12th ult., at our Observatory with the following results:

Mean time of Observation.	Geocentric Long. of	Geocentric Lat. of
	° ′ ″	° ′ ″
July 4.39260	129 31 32	53 25 51
" 8.40480	155 48 12	60 55 24
" 12.37426	171 31 42	62 7 7

From these I have computed the following elements of its orbit:—

	° ′ ″
Long. of ascending node,	278 57 52
Inclination of orbit,	85 38 7·5
Long. of perihelion,	248 30 31
Perihelion distance,	Log. 9·91248
Perihelion passage, 1861, June 11.12177.	Washington M. T.

Motion direct.

Haverford Observatory, West Haverford, Pa., 8th mo. 15th, 1861.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY.

Technical Chemistry.—

1. *On the Coloring Matters Derived from Coal-Tar;* by Mr. H. W. Perkin, F.C.S. (A lecture delivered before the [London] Chemical Society on Thursday, May 16, 1861); from the [London] Journal of Gas Lighting, etc. p. 447, June 18, 1861.—The subject which I have the honor of bringing before you this evening is one which is remarkable for the very rapid progress it is making. Previous to the year 1856, the coloring matters derived from coal-tar products were practically unknown; but, owing to recent discoveries, they are now enumerated among the most important coloring matters employed. It is not my intention to enter into a discussion respecting the numerous patents that have been taken out for the prodution and application of coal-tar colors, nor yet to enter into the practical details of their manufacture; but to notice more particularly their various chemical characters, and also to speak of the methods employed for their application to the arts. I shall endeavor to bring before you all the principal colored products obtainable from the derivatives of coal-tar—both those which are practically employed by the dyer and printer, as well as those which still remain as chemical curiosities only. I may mention that the latter class of compounds is more extensive than the former, and also that many of the coloring matters in it are likely to remain so, for reasons to be mentioned presently.

The coal-tar products which have, up to the present, yielded coloring matters, are numerous. The following is a list of the most important, which are obtained either directly or indirectly from coal-tar:—namely, aniline and its homologues, carbolic acid, chinoline or quinoline, naphthaline, and pyrole bases.

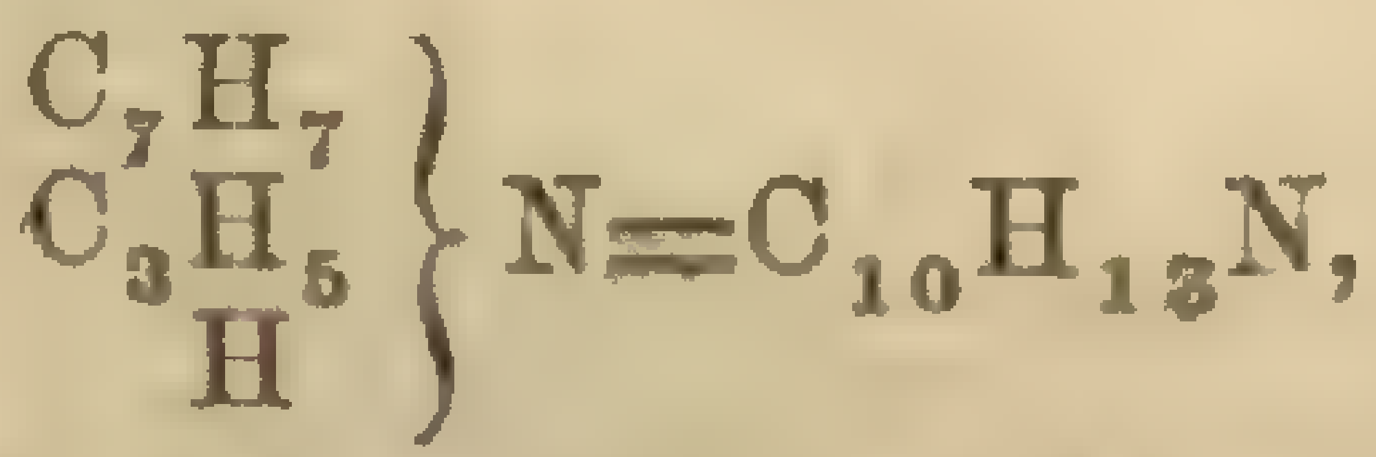
I will commence by speaking of the first and most important of these products, and its derivatives—namely, Aniline.

Aniline was discovered in 1826 by Unverdorben. The original method for its preparation was by digesting indigo with hydrate of potassa, and subjecting the resulting product to distillation. Aniline was also obtained from the basic oils of coal-tar; but the process which is now employed for its preparation is a remarkable instance of the manner in which abstract scientific research becomes, in the course of time, of the most important practical service. It was Faraday who first discovered benzole; he found it in oil-gas. After this, it was obtained by distilling benzoic acid with baryta, which result determined its formula, and was the cause of it being called *benzole.* After this, Mansfield found it to exist in large quantities in common coal-tar naphtha, which is the source from which it is now obtained in very large quantities. Benzole, when studied in the laboratory, was found to yield, under the influence of nitric acid, nitrobenzole. Zinin afterwards discovered the remarkable reaction which sulphid of ammonium exerts upon nitrobenzole, converting it into aniline. And, lastly, Béchamp found that nitrobenzole was converted into aniline when submitted to the action of ferrous acetate. It is Béchamp's process which is now employed for the preparation of aniline by the ton. Had it not been for the investigations cited above, the beautiful aniline colors now so extensively employed, would still remain unknown. When I discovered aniline-purple, nitrobenzole and aniline were only to be met with in the laboratory; in fact, half a pound of aniline was then esteemed quite a treasure, and it was not until a great deal of time and money had been expended that I succeeded in obtaining this substance in large quantities, and at a price sufficiently low for commercial purposes.

The coloring matters obtainable from aniline are numerous. They are the following:—Aniline purple, violine, roseine, fuchsine, alpha aniline purple, bleu de Paris, nitroso-phenyline, dinitraniline, and nitro-phenyline diamine.

Aniline Purple, generally known as Tyrian Purple, Mauve, Phenamine, Indisine, &c.—It has been known for many years that the hypochlorites react on aniline and its salts, producing a purple-colored solution; in fact, hypochlorites are the distinguishing test for aniline; but nothing definite was known of this purple-colored solution, it being simply stated that aniline produced, with hypochlorites, a purple-colored liquid, but that this color was very fugitive. As many very absurd statements have been made respecting the discovery of aniline purple, I will just briefly mention how it was that I first became acquainted with it.

In the early part of 1856, I commenced an investigation on the artificial formation of quinine. To obtain this base, I proposed to act on toluidine with iodid of allyle, so as to form allyle-toluidine, which has the formula—



$$\left.\begin{matrix} C_7H_7 \\ C_3H_5 \\ H \end{matrix}\right\} N = C_{10}H_{13}N,$$

thinking it not improbable that, by oxydizing this, I might obtain the desired result; thus—

$$\underbrace{2(C_{10}H_{13}N) + O_3}_{\text{Allyle-toluidine.}} = \underbrace{C_{20}H_{24}N_2O_2 + H_2O.}_{\text{Quinine.}}$$

For this purpose, I mixed the neutral sulphate of allyle-toluidine with bichromate of potassa; but, instead of quinine, I obtained a dirty reddish-brown precipitate. Nevertheless, being anxious to know more about this curious reaction, I proceeded to examine a more simple base under the same circumstances. For this purpose, I selected aniline, and treated its sulphate with bichromate of potassa. This mixture produced nothing but a very unpromising black precipitate; but, on investigating this precipitate, I found it to contain the substance which is now, I may say, a commercial necessity—namely, aniline purple.

The method adopted for the preparation of analine purple is as follows:—Solutions of equivalent proportions of sulphate of aniline and bichromate of potassa are mixed, and allowed to stand till the reaction is complete. The resulting black precipitate is then thrown on a filter, and washed with water until free from sulphate of potassa. It is then dried. This dry product is afterwards digested several times with coal-tar naphtha, until all resinous matter is separated, and the naphtha ceases to be colored brown. After this, it is repeatedly boiled with alcohol to extract the coloring matter. This alcoholic solution, when distilled, leaves the coloring matter in the bottom of the retort as a beautiful bronze-colored substance.

The aniline purple prepared according to the process just described, although suitable for practical purposes, is not chemically pure. If required pure, it is best to boil it in a large quantity of water, then filter the resulting colored solution, and precipitate the coloring matter from it by means of an alkali. The precipitate thus obtained should be collected on a filter washed with water until free from alkali, and dried. When dry, it is to be dissolved in absolute alcohol, the resulting solution filtered, and then evaporated to dryness over the water-bath. Thus obtained, aniline purple appears as a brittle substance, having a beautiful bronze-colored surface; but, if some of its alcoholic solution be evaporated on a glass plate, and viewed by transmitted light, it appears a beautiful bluish violet color. If considerable quantities of an alcoholic solution of the coloring matter, containing a little water be evaporated to dryness, the surface of the coloring matter next to the evaporating dish, when detached, often possesses a golden-green appearance. Aniline purple is difficultly soluble in cold water, although it imparts a deep purple color to that liquid. It is more soluble in hot water, but its hot aqueous solution, when left to cool, assumes the form of a purple jelly. It is very soluble in alcohols, though nearly insoluble in ether and hydrocarbons. Aniline dissolves it readily. In properties it seems to be slightly basic, as it is more soluble in acidulated than in pure water. Alkalies and saline substances precipitate it from its aqueous solution as a dark purplish-black powder. Bichlorid of mercury precipitates it in a very finely-divided state. A little of this precipitate, which appears to be a double compound of chlorid of mercury and coloring matter, when suspended in water and viewed by transmitted light, appears of a blue or violet color. If a small quantity of hydrate of potassium or sodium be added to an alcoholic solution of the coloring matter, it causes it to assume a violet tint, but without effecting any change in the coloring matter itself. Ebullition with alcoholic potash does not decompose it. Aniline purple dissolves in concentrated sulphuric acid, forming a dirty-green solution. This, when slightly diluted, assumes a beautiful blue color. Excess of water restores it to its original purple color. I have had a specimen of this coloring matter heated for an hour to 100° centigrade, with Nordhausen sulphuric acid without suffering decomposition, being restored to its original color by means of water, and possessing precisely the same properties as it had before being subjected to this powerful agent. Hydrochloric acid acts upon it in the same manner as sulphuric acid. It is decomposed by chlorine, and also by fuming nitric acid. Bichlorid of tin is without action upon it. Powerful reducing agents have a peculiar action upon this coloring matter, somewhat analagous to the action of reducing agents on indigo. An alcoholic solution of sulphid of ammonium, when mixed with an alcoholic solution of the coloring matter, causes it to assume a pale, slightly brownish color. This solution, when brought in contact with the atmosphere, instantly assumes its beauty and intensity of color. An alcoholic solution of the coloring matter, when mixed with a little protoxyd of iron, changes to a pale-brown color. This solution also becomes purple when exposed to the action of the atmosphere. Sulphurous acid does not affect the color of this substance.

The coloring matter forms a remarkable compound with tannin. When an aqueous solution of the coloring matter is mixed with a solution of the tannin, precipitation takes place. The precipitate thus formed, after having been well washed, no longer possesses the properties of the pure coloring matter. It is insoluble in water. Like the pure coloring matter, it dissolves in concentrated sulphuric acid, forming a dirty-green liquid; but, on adding an excess of water to this solution, the new compound is precipitated unchanged. This compound is rather duller in color than the pure coloring matter itself. Aniline purple, when agitated with a little moist binoxyd of lead, is transformed into roseine. This coloring matter is remarkable for its intensity—a few grains of it coloring a considerable quantity of spirits of wine.

Violine.—This coloring matter, which is a product of the oxydation of aniline, was first obtained by Dr. David Price. He prepared it by heating an aqueous liquid containing two equivalents of sulphuric acid, and one equivalent of aniline, to the boiling-point, and then adding one equivalent of binoxyd of lead, boiling the mixture for some time, and filtering it whilst hot. The filtrate, which is of a dark purple hue, is boiled with potash, both to separate the excess of aniline, and also to precipitate the coloring matter. When all the free aniline is volatilized, the residue is thrown on a filter, and slightly washed with water, and then dissolved in a dilute solution of tartaric acid. This solution after filtration is evaporated to a small bulk, refiltered, and then precipitated by means of an alkali. Thus obtained, violine presents itself as a blackish-purple powder, which, when dissolved in alcohol, and evaporated to dryness, appears as a brittle, bronze-colored substance, similar to aniline purple, but possessing a more coppery-colored reflection. It is more insoluble in water than the preceding coloring matter; it is very soluble in alcohol, but insoluble in ether and hydrocarbons. These solutions possess a color somewhat similar to that of the field violet. Concentrated sulphuric acid dissolves it, forming a green solution, but excess of water restores it to its original color. Like aniline purple, reducing agents deprive it of its color, but which is restored by the action of the atmosphere. Tannin produces an insoluble compound with it; when agitated with a small quantity of binoxyd of lead, it is converted into aniline purple; excess of this reagent changes it into roseine.

Roseine.—This substance nearly always accompanies aniline purple, though in very small quantities. It was first noticed publicly by C. Greville Williams, and afterward by Dr. David Price. Williams used manganates for its preparation, but Dr. David Price prepared it by means of binoxyd of lead. His process is as follows:—To a boiling solution of one equivalent of sulphate of aniline, two equivalents of binoxyd of lead are added, and the mixture boiled for a short time. The rose-colored solution is then filtered, and the filtrate evaporated to a small bulk, which causes a certain amount of resinous matter to be separated. This evaporated solution is then filtered, and the coloring matter precipitated by means of an alkali; it is then collected on a filter, slightly washed, and then dried. The coloring matter thus prepared readily dissolves in alcohol, forming a fine crimson-colored liquid, which, when evaporated to dryness, leaves the coloring matter as a dark brittle substance, having a slightly metallic reflection. It is much more soluble in water than either aniline purple or violine; but, like them, it is insoluble in hydrocarbons, and is more soluble in acids than in neutral liquids. Concentrated sulphuric acid dissolves it, forming a green solution; excess of water restores it to its original color. It forms a compound with tannin, and is also deodorized, or nearly so, by powerful reducing agents.

The three coloring matters just mentioned, namely, aniline purple, violine, and roseine, are evidently closely allied, for they have nearly the same properties. They are all formed under similar circumstances, namely, by the action of oxydizing agents in the presence of water; they are all slightly soluble in water, though as the shade of color becomes redder, so their solubility increases; alkalies precipitate them from their aqueous solutions; concentrated sulphuric acid dissolves them, forming green solutions, which excess of water restores to the original color of the coloring matters; powerful reducing agents deprive them of their color, or nearly so; but it is again restored by the influence of oxygen; and, lastly, tannin forms insoluble compounds with them all.

Fuchsine or Magenta.—This beautiful product, which is often improperly called roseine, is a member of an entirely different series of compounds from the foregoing, being formed under very different circumstances, and possessing very different properties. This coloring matter was first observed by Natanson, in 1856, when studying the action of chlorid of ethylene on aniline, and afterwards, shortly before it was practically introduced into the arts, by Dr. Hofmann, when preparing cyantriphenyle-diamine by the action of bichlorid of carbon on aniline. It was Mr. Verguin who first brought it forward as a dyeing agent, and who, I believe, taught manufacturers how to prepare it on the large scale. Fuchsine is invariably formed at a temperature ranging from 170° to 190° centigrade. It is produced from aniline by the action of reducible chlorinized, brominized, iodized, or fluorized substances, as well as by weak oxydizing agents. The substances used for its preparation on the large scale are perchlorids of tin and of mercury, and the nitrates of mercury. It has also been prepared with bichlorid of carbon.

Preparation of Fuchsine by the Action of Bichlorid of Tin on Aniline.—Aniline combines with bichlorid of tin, evidently producing a double compound. This product is a white substance, and may be prepared by adding to aniline, bichlorid of tin in the anhydrous state, or dissolved in water. Anhydrous bichlorid of tin combines with aniline with great energy to form this compound. To prepare fuchsine from this double compound, it is necessary that it should be free from water, or nearly so; therefore, anhydrous chlorid of tin is generally employed for its preparation. The process adopted is as follows:—Anhydrous bichlorid of tin is slowly added to an excess of aniline, the mixture being constantly stirred, and the pasty mass thus formed gradually heated. As the temperature increases, it becomes quite liquid and also brown in color. As soon as the temperature nearly approaches the boiling point of the mixture, it rapidly changes to a black-looking liquid which, when viewed in thin layers, presents a rich crimson color. This is kept at its boiling point some time, and then well boiled with a large quantity of water. By this means, the principal part of the coloring matter is extracted, together with considerable quantities of hydrochlorid of aniline. The residue is a solid of brown color, and contains considerable quantities of tin in the form of a proto-compound. The aqueous solution of the coloring matter and hydrochlorate of aniline is then boiled, so as to volatilize any free aniline it may contain, and then saturated with chlorid of sodium. The chlorid of sodium causes the coloring matter to separate as a semi-solid pitchy substance, of a golden-green aspect, while the hydrochlorate of aniline remains in solution. The coloring matter thus obtained may be further purified by digestion with benzole, which dissolves out a certain amount of resinous matter.

Preparation of Fuchsine by the Action of Nitrate of Mercury on Aniline.— When protonitrate of mercury is left in contact with aniline for some time it forms a white pasty mass; but when carefully heated to 170° or 180° centigrade, it reacts upon it, forming a brown liquid, which gradually changes till of a dark crimson color. At the same time, the whole of the metal of the mercury-salt collects at the bottom of the vessel the experiment is conducted in. This product, when separated from the metallic mercury and allowed to cool, becomes semi-solid, being filled with crystals of nitrate of aniline. To purify this product, it is best to dissolve out the nitrate of aniline it contains in a small quantity of cold water, and then to boil the remaining product several times with fresh quantities of water until the principal of the coloring matter is extracted, and filter the resulting aqueous solutions while hot. On cooling, the solutions will deposit the coloring matter as a golden-green tarry substance, from which benzole separates a small quantity of a brown impurity, leaving the coloring matter as a brittle solid.

I have briefly described the above processes, because they may to some extent be regarded as types of most of the methods employed for the production of this coloring matter—the first representing its formation by the action of reducible chlorids upon aniline, and the latter by the influence of weak oxydizing agents.

Fuchsine is undoubtedly an organic base, and a more powerful one than is generally supposed. The product obtained from aniline by means of bichlorid of tin is hydrochlorate of fuchsine, and that obtained by the oxydizing action of nitrate of mercury is the nitrate of fuchsine. My reason for stating this is, that, on examining the coloring matter obtained by chlorid of tin, I found it to contain large quantities of combined hydrochloric acid, and, when nitrate of mercury was used, considerable quantities of combined nitric acid; therefore, I concluded that the former is the hydrochlorate, and the latter the nitrate.

Fuchsine is separated from its salts by precipitation with a small quantity of ammonia. When freshly precipitated, fuchsine is a red bulky paste, which, when dry, contracts, forming a purplish red powder. It is difficultly soluble in water, though tolerably soluble in alcohol; it is not soluble in ether or hydrocarbons; a small quantity of hydrochloric acid causes it to dissolve freely in hot water; but an excess of either hydrochloric acid or sulphuric acid dissolves it, forming a brownish-yellow liquid from which ammonia separates it unchanged. By this reaction it may be distinguished from roseine, which dissolves in strong sulphuric acid, producing a green liquid. Caustic alkalies, or ammonia in excess, partially precipitate fuchsine from its salts; but, at the same time, dissolve a considerable quantity of it, forming nearly colorless liquids.

Acetic acid added to these alkaline solutions restores the color of the fuchsine; and, if the liquids are concentrated, the base precipitates as a red, flocculent substance. An alcoholic solution of fuchsine, when evaporated to dryness, leaves the coloring matter as a brittle mass, having a remarkably beautiful golden-green metallic reflection. By transmitted light it has a red color. Mr. Béchamp has analyzed carefully-prepared specimens of fuchsine, and found it to have the formula $C_{12}H_{12}N_2O$. This hydrochlorate he found to contain an amount of hydrochloric acid, corresponding with the formula $C_{12}H_{12}N_2O$, H Cl. He also examined the hydrochloroplatinate, which is a purple precipitate; it has the formula $C_{12}H_{12}N_2O$ H Pt Cl_3. The existence of oxygen in this base is remarkable, because, in many instances, it is produced from agents which do not contain a trace of oxygen—as, for example, bichlorid of tin and aniline. The only way I can account for the presence of oxygen in the product analyzed is, that it was a hydrate; thus:—

$$\underbrace{C_{12}H_{12}N_2O}_{\text{Béchamp's fuchsine.}} = \underbrace{C_{12}H_{10}N_2}_{\text{Anhydrous fuchsine.}} + \underbrace{H_2O}_{\text{Water.}}$$

This is, perhaps, to some extent, confirmed by an experiment I made with iodaniline. I find that iodaniline, when heated, yields fuchsine. This change can only be expressed thus:—

$$\underbrace{2(C_6[H_6I]N)}_{\text{Iodaniline.}} = \underbrace{C_{12}H_{10}N_2}_{\text{Anhydrous fuchsine.}} + 2HI$$

But, supposing the fuchsine examined by Mr. Béchamp to have been a hydrate, it is remarkable that its hydrochlorate, and, more particularly, its hydrochlorplatinate, should also be hydrates. But, as our knowledge of this body is as yet but scanty, we must wait for the accumulation of facts before we can form any fixed opinion respecting its constitution. The compounds investigated by Mr. Béchamp appear to be uncrystallizable. Some time back, I had upwards of 100 gallons of a hot, aqueous solution of this coloring matter, which had been prepared by means of nitrate of mercury. This solution, on standing until cold, deposited a considerable quantity of coloring matter in the form of small octahedra, having a most beautiful green metallic reflection. Reducing agents decolorize fuchsine, but the oxygen of the atmosphere restores it to its original color. If an alcoholic solution of fuchsine be left in contact with sulphid of ammonium until decolorized, or nearly so, and then exposed to the atmosphere, its color is immediately restored; but, if left to stand for several days, and then submitted to the atmosphere, several hours elapse before it assumes its original color. Like aniline purple, fuchsine is a very intense coloring matter. Tannin precipitates both fuchsine and its salts, forming difficultly soluble substances. Bichlorid of mercury also precipitates this base and its salts, forming double compounds. When preparing fuchsine by means of bichlorid of tin, or more particularly when using nitrate of mercury, there are two other coloring matters produced, one possessing an orange color, and the other a purple hue. It is on account of the presence of the former that some samples of commercial fuchsine possess a scarlet character of color. Of this orange-coloring matter, scarcely anything is known. It is not precipitated from these salts by means of alkalies. Of the purple-coloring matter, little is also known. It is formed at the same time as fuchsine, but in very small quantities, and from which it is difficult to separate perfectly. Its properties are similar to those of fuchsine. It dissolves in concentrated sulphuric acid, forming a reddish-brown solution, from which part of the coloring matter is separated by dilution with water. Hydrochloric acid dissolves it, forming a dirty, yellowish-red liquid.—(*To be continued.*)

2. *Action of Sulphate of Copper when employed as a preservative of wood.*—Kœnig has investigated the chemical reactions which occur when wood is impregnated with a preservative solution of blue vitriol. He finds as a general rule, that a certain quantity of basic sulphate of copper remains combined in the pores of the wood in such a manner that it cannot be washed out with water. The copper salt may be seen by its green color in the spaces between the yearly rings in the less compact portions of the wood, that is to say in those portions which contain the sap. Those varieties of wood which contain the most resin retain the largest amount of the copper salt,—oak, for example, retaining but little of it. The ligneous fibre itself appears to have little or nothing to do with the fixation of the copper salt, and indeed none whatever is retained in chemical combination, so that it cannot be washed out with water, by pure cellulose. When wood from which all resin has been extracted by boiling alcohol, is impregnated with sulphate of copper, it does not become colored like the original resinous wood, and the copper salt contained in it may readily be washed out with water. In like manner, from impregnated resinous wood all the copper salt may be removed, with the resin, by means of alcohol.

The constituents of the blue vitriol are consequently fixed in the wood by means of the resin which this contains.

Further it is found that the impregnated wood contains less nitrogen than that which is unimpregnated and that it is even possible to remove all the nitrogenous components of the wood by long continued treatment with the solution of sulphate of copper. The nitrogenous matters being soluble in an excess of this solution just as the precipitate which forms when aqueous solutions of albumen and sulphate of copper are mixed is soluble in excess of the latter. Since the nitrogenous matters are well known to be promoters of putrefaction, their removal readily accounts for the increased durability of the impregnated wood.

The author hopes to explain in a similar manner the action of other salts, like chlorid of zinc, &c., which are used for preserving timber, and is now engaged in investigating the question.

The utility of blue vitriol as a preservative may also depend in a measure upon the resinous copper salt which is formed, by which the pores of the wood are more or less filled up and the ligneous fibre covered so that contact with the air is prevented and the attacks of insects hindered. It is suggested that those cases in which the anticipated benefits have not been realized in practice by impregnating wood with a solution of blue vitriol, may probably be referred to the use of an insufficient amount of this agent—i. e., where the wood was not immersed in the solution for a sufficient length of time. The action should be one of lixiviation, not merely of absorption.—*Programm der Realschule zu Leipzig*, 1861; Bœttger's *polyt. Notizblatt*, xvi, 177, (1861, No. 12).

3. *Ozone as a means of restoring old and faded engravings, etc.*—According to v. GORUP-BESANEZ, ozone when properly applied is a most effective and convenient agent for restoring books or prints which have become brown by age, or been soiled or smeared with coloring matter; only a short time being required to render them perfectly white, as if just from the press, and this without injuring in the least the blackness of the printer's ink or the lines of crayon drawings.

As examples of his results the author mentions a book of the sixteenth century upon a page of which several sentences had been painted over, by the monks of that epoch, with a black, shining coloring matter in order to render them illegible, and of which no trace of a line could be detected. After 36 hours treatment with ozone the coloring matter was entirely destroyed, and the most careful scrutiny of the page would have failed to discover that any of the lines had once been painted over. In like manner a wood cut of Durer which had been besmeared with a dark yellow color was completely restored.

Writing ink may be readily discharged by ozone, especially if the paper be subsequently treated with very dilute chlorhydric acid to remove the oxyd of iron.

Printer's ink is not attacked by ozone to any appreciable extent unless the action be long continued. Vegetable coloring matters are completely removed by it, but metallic coloring matters, grease spots and stains produced by fungi cannot thus be destroyed.

As applied in the small way, the method consists in placing a bit of phosphorus about 3 inches in length and ½ an inch in diameter, the surface of which has been scraped bright, in a wide necked glass carboy, or other large hollow vessel, pouring in as much water, at about 30° (C.), as will half cover the phosphorus, closing the vessel with a cork, and allowing the whole to stand until the jar is charged as strongly as possible with ozone, which ordinarily occurs after 12 or 18 hours. Then without removing the phosphorus or water, the paper to be bleached, which has been moistened with water, rolled up, and fastened to a platinum wire in a suitable manner, is hung in the middle of the vessel. The cork is now restored and the apparatus left to itself. The roll of paper is soon surrounded with the fumes arising from the phosphorus and the stains gradually disappear. The rapidity of the operation of course depends upon the nature of the substance to be discharged—three days having been the longest time required in any of the experiments. Prints which had merely become brown by age and those stained with coffee usually become perfectly white and clean in the course of 48 hours. The action of the ozone however must not be continued too long lest some of the finer lines of the engraving should be injured. After all the spots have disappeared, the paper is strongly acid and if allowed to dry when in this condition would become exceedingly brittle and also dark colored. It is consequently necessary to remove the acid completely. In order to accomplish this the paper is placed in water which is frequently renewed and allowed to lie there until a bit of blue litmus paper pressed against it is no longer reddened. The paper is then passed through water to which a few drops of a solution of soda have been added and is spread upon a glass plate, this is slightly inclined and a fine stream of water is allowed to flow over the paper during 24 hours. After the paper, on exposure to the air, has become dry enough to remove from the glass without danger of tearing, it is taken off and pressed dry between folds of filter paper.

The author remarks that in case the process were attempted on a larger scale it would probably be well to have glass troughs or boxes blown of the desired form, since it is not easy to prepare suitable vessels by any process of fastening together pieces of glass, the cement being attacked by ozone.

Attempts to apply ozone in restoring oil paintings gave only negative results, the action having been irregular.—*Annalen der Chemie und Pharmacie*, cxviii, 232.

4. *New Anæsthetic.*—During the past few months considerable interest has been excited among members of the medical profession by an attempt to introduce into practice a volatile liquid possessing anæsthetic properties which is obtained as an incidental product in the manufacture of coal oil. Of the chemical history of this substance—called keroselene by its manufacturers, but little is as yet known. Prof. Bacon, of the Harvard Medical School, informs us "that a sample in his possession is of sp. gr. 0·640, at 72° F. When heated in a flask containing scraps of platinum foil it began to boil at about 85° F. As the more volatile parts distilled off, the temperature continued to rise, and at 170° about three-quarters of the liquid had evaporated. It continued to boil feebly, but the whole was not converted into vapor until the thermometer had risen considerably above 300°; and when the flask was allowed to cool, much of the vapor condensed before the temperature had fallen to 300°. It is evident that several, perhaps many, hydrocarbons are present, having a wide range of boiling points. Probably the most volatile of them would be gaseous at ordinary temperatures, if isolated. It is remarkable that the keroselene should be so readily and completely volatile at atmospheric temperatures. I found that keroselene and Squibb's ether, exposed in watch glasses, lost equal weights in 2½ and 3½ minutes respectively; and the former evaporated completely in about two-thirds of the time required for the ether. The specimen which I examined contained a little *sulphur.* Some sulphur compound was therefore present as an impurity, which would be decidedly objectionable for anæsthetic purposes."

The vapor of this substance possesses very decided anæsthetic properties. This was first accidentally noticed by its effects upon a laborer engaged in cleaning a cistern at a coal-oil manufactory, and afterwards proved by the workmen by experiments upon flies and mice. Whether it can be employed without danger as a substitute for ether or chloroform is as yet undecided. Dr. H. J. Bigelow (Boston Medical and Surgical Journal (July 11, 1861) lxiv, 494,) reports several cases in which its exhibition was attended with unfavorable symptoms; and at the present time the general feeling of medical men in Boston with regard to its value is evidently much less favorable than when it was first brought forward. It may be remarked that the "keroselene" in question is exceedingly well purified as far as relates to its odor, being almost entirely free from the objectionable smell which characterizes most of the light coal oils. F. H. S.

Boston, Aug. 20.

ANALYTICAL CHEMISTRY.—

5. *Reinsch's test for Arsenic not applicable to solutions containing Arsenic acid.*—WERTHER finds that the practice of applying Reinsch's test immediately to the liquid obtained by the action of chlorate of potash and chlorhydric acid upon the substance suspected to contain arsenic must be discarded and that, as a general rule, the test is inapplicable to liquids which contain arsenic acid.

It had heretofore been supposed, and the belief is countenanced by so high an authority as Fresenius (*Anleitung zur qualitativen chem. Analyse*, 10te *Aufl.*, p. 148, §132, No. 7), that arsenic acid would readily be reduced by the copper employed in the test and that the products of this reduction would not interfere with the deposition of the characteristic arsenical coating upon the copper. But Werther now shows that solutions either of free arsenic acids, or of its salts in chlorhydric or sulphuric acid deposit no coating upon bright metallic copper unless a considerable quantity of arsenic acid be present. Similar negative results were obtained from solutions which were allowed to stand upon the copper in the cold for months and from others which were boiled at longer or shorter intervals.

It is consequently with arsenious acid alone that the characteristic reaction of arsenic can be exhibited when dilute solutions of the latter are boiled with copper and chlorhydric acid.

In the experiments of the author no unequivocal, lustrous, arsenical coating upon the copper could be obtained, on boiling for a few minutes, from acid solutions of arseniate of magnesia and ammonia unless these contained at least 0·4 à 0·5 grm. of arsenic in 50 cc.; although dark colored coatings of uncertain composition, from which no arsenic could be obtained by volatilization in a current of hydrogen or of carbonic acid, were obtained from more dilute solutions.

In judicial investigations, therefore, Reinsch's test should never be employed upon the liquid resulting from the oxydation of organic substances, until the arsenic acid therein contained has been reduced by a current of sulphurous acid.

Having observed that the dubious black coating sometimes obtained from dilute solutions of arsenic acid, as just described, frequently disappeared again as he sought to increase it by longer continued action, the author was led to the observation that not only the coating in question but pure metallic arsenic is gradually dissolved by chlorid of copper when this is gently heated. After having acted during a few hours the solution contains considerable quantities of arsenic acid but scarcely a trace of arsenious acid.

On the other hand common chlorhydric acid diluted with an equal volume of water being boiled upon bright metallic arsenic dissolved none of it, nor was any dissolved after digesting for two days at 50° à 60° (C.) in a closed vessel.—*Journal für praktische Chemie*, lxxxii, 286.

II. GEOLOGY.

1. *Reply to Mr. Marcou's strictures on Mr. F. B. Meek in his Notes on the Cretaceous and Carboniferous Rocks of Texas*, (in a letter to the Editors.)—GENTLEMEN:—I had intended never to answer any of Mr. Marcou's papers on the disputed points in the geology of the far-west, excepting at such times as I could do so in connection with the announcement of newly discovered facts. To prolong a discussion of this kind by merely reiterating opinions already expressed, after all the facts in possession of the parties have been published, can neither advance the interests of science, nor prove very interesting to scientific readers. In a paper recently published by Mr. Marcou, however, he makes some statements of a personal nature, which my friends think, if allowed to pass unnoticed, may place me in a false position.

The paper to which I allude is entitled, "Notes on the Cretaceous and Carboniferous rocks of Texas, by Jules Marcou,"—and was published in the VIIIth volume of the Proceedings of the Boston Society of Natural History. It is intended as a critical review of a paper by Dr. B. F. Shumard, containing a section of the Texas formations; and the personal remarks to which my attention has been called, are made incidentally in connection with this review.

The first of these remarks worthy of notice, (and it is of no very great importance,) occurs on page 94, where he mentions an error on one of the plates of the VIIth volume of the Pacific Railroad Reports, in copying a figure from a paper of his in the Bulletin of the Geological Society of France, citing it as an instance of the careless work of "Messrs. Hall, Blake and Meek."

Now the fact is, the figures referred to by Mr. Marcou, were not copied by me, as I have already explained to him by letter. The other figures on the same plate were drawn by me from specimens left by Mr. Blake at Albany, N. Y., where I was then living. Each of my drawings was on a separate piece of paper when they went out of my hands. The other figures from Mr. Marcou's paper, were copied and arranged on the same plate with them by some person at Washington City; and it was not until I saw a proof of the plate sent on to Albany, that I was aware these figures had been copied from Mr. Marcou's paper.

After the publication of the volume containing this plate, I received a letter from Mr. Marcou, who was then in Switzerland, complaining that I had received credit for drawing a plate in it which contained a few figures that had been copied from a paper of his. To this I replied, informing Mr. Marcou that the figures referred to by him, were not copied by me, and that I was not aware my name would be mentioned in connection with *any* of the drawings in this report, until I saw it in print.

As I received an answer from Mr. Marcou (dated Zurich, July 1st, 1858, which is now in my possession) acknowledging the reception of my letter, and expressing satisfaction with the explanation in regard to the plate, I am a little surprised that he should now connect my name with it; though it is possible the nature of our correspondence on the subject may have escaped his memory.

On page 96 of his Boston paper already alluded to, Mr. Marcou, in speaking of the so-called *Gryphæa Tucumcarii*, says, "On looking more closely at the plates of the *Mexican Boundary* Report, I found on the last plate, No. XXI, fig. 3a, 3b, 3c, a specimen of *Gryphæa Tucumcarii* under the false name of *Gryphæa Pitcheri.* Mr. Conrad in his description of *Gryphæa Pitcheri*, p. 155, makes no reference whatever to that plate, nor to the figures 3a, 3b, 3c; and in the explanation of Prof. Hall's Report, p. 174, nothing is said of the locality or the stratigraphical position of this fossil. The plate was drawn by Mr. F. B. Meek, who has put it under the head of *Cretaceous.*"

He even goes on to intimate that I had either myself, or in connection with another party, added the figures of this fossil to Mr. Conrad's report, without his knowledge or consent; and that too (as he thinks) under a wrong name, and in a wrong place. He also quotes a letter of Mr. Conrad's in such connection as to convey the impression that Mr. Conrad intended to intimate the same thing. Thus he says, "desirous to know the opinion of Mr. Conrad on this incomprehensible and doubtful proceeding, I wrote him, and give below his answer:"—

"Philadelphia, January 25th, 1861.

JULES MARCOU, Esq.

Dear Sir. . . . . When I drew up the report in Emory's Survey, I was shown by Prof. Hall a series of *Gryphæa*, some of which were undoubtedly your *G. Tucumcarii* as figured on plate XXI. Prof. Hall thought they graduated into *G. Pitcheri*, and I thought so too at that time. The name of your species ought not to have been placed as a synonym to plate VII, figure 3, for it is undoubtedly *G. Pitcheri.*

But the figures on plate XXI, represent a species and specimen, the locality of which is *unknown* to me, and were engraved *after* I had sent in my report and descriptions, so that I cannot now say that I do know whether *G. Pitcheri* is identical with your species or not.

The locality of the *G. Pitcheri* (page 155 Leon Springs, Texas; Plains of Kiamesha, Arkansas; New Braunfels, Texas; Fort Washita and Cross Timbers, Texas;) are correctly given from MSS. accompanying the specimens.

. . . . Sincerely yours, T. A. CONRAD."

Having had nothing whatever to do with the Mexican Boundary Report, excepting to draw the fossils figured in it, while all my drawings were on separate pieces of Bristol board, none of which were arranged on the plates by me;—and having had no agency whatever in deciding which figures should be arranged under the head of Cretaceous or otherwise,—I knew it was utterly impossible that Mr. Conrad could have even suspected, when he was writing his letter quoted above, the nature of the construction Mr. Marcou would place upon it. Consequently on seeing Mr. Marcou's paper I wrote Mr. Conrad on the subject, and received the following reply:—

"Trenton, June 21, [1861.]

My Dear Sir,—I am surprised that your name has been introduced in reference to the Palæontological report in Emory's Boundary Survey. You certainly had no connection with it other than drawing the plates, and never, that I know of, expressed an opinion of any species named in that report. When I said that *G. Tucumcarii* ought not to have been placed as a synonym to plate VII, fig. 3, I meant that *I* should not have done so, as I was not entirely satisfied of the identity of *G. Pitcheri* and *G. Tucumcarii.* I alone am responsible for the error, if there be one. I either made the reference myself, or authorized Prof. Hall to make it in my name. The figure on plate XXI, to the best of my recollection was engraved *after* I had sent in my report, but no one excepting myself is responsible for omitting any reference to it in my report.

Truly yours, T. A. CONRAD.

In a postscript to this letter Mr. Conrad also states, amongst other things, that he is now satisfied that the so-called *G. Tucumcarii* is the typical form of Morton's Cretaceous *G. Pitcheri.* The following are his words: "by-the-by, Gabb showed me specimens which convince me that *Tucumcarii* is the original form of Morton's *G. Pitcheri.*"

So it will be seen that what Mr. Marcou calls "the mysterious appearance of this beautiful fossil" under the head of Cretaceous, in Mr. Conrad's report, is not, after all, so "incomprehensible and doubtful" a matter as he had supposed.

Mr. Conrad's latest opinion, after examining additional specimens that the so-called *G. Tucumcarii* is the typical form of Morton's *G. Pitcheri,* and that his own *G. Pitcheri* var. *navia,* (that is, the narrow form Dr. Roemer and Mr. Marcou referred to *G. Pitcheri*) is probably a distinct species, is in exact accordance with my own views expressed as long back as 1858. Mr. Gabb has also arrived at the same conclusion;* and when it is borne in mind that he and Mr. Conrad have at Philadelphia Morton's original specimen, and an extensive series of the *Gryphœas* of various ages from Texas and Arkansas, for comparison with Mr. Marcou's figures, drawn as he states by one of the best artists in Paris, the impartial reader will readily understand the value of their opinion on this point.

Mr. Marcou lays great stress upon the differences between the so-called *G. Tucumcarii,* and the narrow form formerly referred by him to *G. Pitcheri,* as if this has some bearing on the question at issue in regard to his Pyramid Mountain Jurassic. Whether we regard these forms, however, as distinct, or only as varieties of one species, has no bearing whatever on the main question at issue, since it is well known, as has been shown by Dr. B. F. Shumard, that they are both Cretaceous, and occur together in the same bed in Texas along with the so-called *Ostrea Marshii,* and numerous well marked Cretaceous fossils. Dr. S. has also shown that undoubted Cretaceous genera and species also occur in Texas, far beneath the horizon of the *Ostrea* and *Gryphœa,* Mr. Marcou supposed to belong to the Jurassic.

In regard to Mr. Marcou's efforts to explain away these facts, and to show that Dr. Shumard is in error as to the order of succession of the Texas formations, it is unnecessary for me to say a single word, as that geologist is abundantly able to defend his own views.

That Jurassic, and possibly Triassic rocks occur in New Mexico and other portions of the southwest, is very probable, but what Dr. Shumard, Dr. Newberry, Mr. Conrad, Mr. Gabb, myself and others maintain is, that the fossils relied upon by Mr. Marcou to prove the existence of the Jurassic at Pyramid mountain, are Cretaceous species; and consequently that he has included Cretaceous strata in his Jurassic. We have undoubted Jurassic fossils from the Black Hills in Nebraska, and from a few other points on the north Platte above Fort Laramie, as well as from some places in Utah, but they all differ widely from those referred by Mr. Marcou to that epoch, and hold a much lower stratigraphical position. No Jurassic fossils have yet been found in the country west of the Mississippi, south of Utah, so far as my knowledge extends, unless some remains of plants, and large Saurian bones discovered by Dr. Newberry in New Mexico, belong to that epoch; and these came from the red beds far below the horizon of Mr. Marcou's supposed Jurassic fossils.

I remain, yours truly, F. B. MEEK.

Washington, D. C., July 27, 1861.

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* **Proceed. Acad. Nat. Sci. Phil. Feb. 1861.**

2. *Note on Mr. Lesquereux's Table of Comparative Sections of Coal Measures.* (To the Editors of the Am. Jour. Sci. and Arts.)—Throughout the notice of the Kentucky Geological Survey, published in the July number of your Journal, my name is perhaps liable to be confounded with that of my brother Joseph Lesley, Jr., to whom is due whatever credit belongs to the Topographico-geological report of the margin of the eastern coal field of Kentucky.* As in this notice, quotations from my friend Mr. Lesquereux refer several times to my little Manual of Coal and its topography, published in 1856, I trust that you will permit me a word or two in relation to its exhibition of the Pennsylvania measures. The sections referred to in the Manual are most of them from ten to twenty years old, and were offered as illustrations of principles, not as guides to local geologists. They were meant to exhibit the general scheme of the Coal Measures; but neither settled the principal questions mooted among us, nor embodied late discoveries. One of these is of such importance that I wish to call special attention to it here, as I have done once already at the Montreal Meeting of the American Association.

It will be noticed in Mr. Lesquereux's "Table of Comparative Sections" accompanying your notice, and in the first two columns representing the Kentucky Coals, that a group of beds, four in number, seem to be represented in the other columns by only one bed, the Pennsylvania Pittsburg bed.

Five years ago I made a careful survey of the Coal series in Somerset County, Pennsylvania, especially of its upper and middle part. I found here a group of beds, one of which was the Pittsburg bed, exactly like the group No. 9, 10, 11 and 12 of Western Kentucky, with the Anvil Rock in character above it. The group consisted of three large beds, ten, twelve and sixteen feet thick respectively, with intervals of fifty and sixty feet between them; and, with these, just under the Anvil Rock, was a fourth and smaller bed. I have never had time to write out for publication my notes of this survey, nor indeed of any of my surveys; but no region that I have examined is more worthy of description, because of the flagrant representation which it makes of the persistency of our coal beds over a region at least seven hundred miles wide from east to west; affording a satisfactory demonstration, that the middle Kentucky area, or Cincinnati Axis as we used to call it, has been denuded of the same coal measures, deposited in the same order, and of about the same thickness as now exist at widely separated points on both sides of it; namely, at the summit of the Alleghany Mountain and at the mouth of the Tennessee river. I have always considered the theory of the dying out of the coal-beds and intervening rocks along the western outcrop of the S.E. Ohio and E. Kentucky coal area to be contrary to sound and wide geological views. I have always doubted or disbelieved the few local data supposed to favor that theory, drawn from the vicinity of Greenup County, Kentucky. My own observations at Hanging Rock were against it. My brother's very complete survey of the outcrop across the whole State of Kentucky shows nothing of it. On the contrary, everywhere along the great escarpment line of the Ohio, Kentucky and Tennessee outcrop, denudation reigns supreme as limiting the coal area westward, without observable assistance from nonconformability. And certainly the exact agreement of the great group of upper beds and Top Rock of Somerset County, Pennsylvania, with the great upper group and Anvil Rock of Western Kentucky, must convince every geologist that one continuous coal area stretched from the head of the Potomac to the mouth of the Tennessee, when the Pittsburg bed and its group were made. I am happy to say that in this faith I agree with Mr. Lesquereux.*

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* Peter Lesley, the author of a "Manual of Coal and its Topography," and one of the original corps of Assistants on the Pennsylvania Geological Survey in 1839—has since 1848 written his name J. P. Lesley—No careful reader of the notice alluded to on p. 118 need be perplexed in distinguishing between this gentleman and his accomplished brother Joseph Lesley, Jr., author of the Topographical Geological Report of Kentucky.—Eds.

There are some other points which I wish to touch on while occasion offers. My friend Mr. Lesquereux wishes to suppress the name False Coal Measures. I agree with him. I never liked the term. I used it because there was no other. In fact no other has been suggested, except in connection with some whole nomenclature, such as Mr. Rogers's Vespertine coal. But then it has a well-known meaning, and is understood by everybody. Geologists always talked of the Coal Measures. But a day came when a whole series of carboniferous rocks were discovered not belonging to the Coal Measures, and in fact thousands of feet below the acknowledged base of the Coal Measures. These rocks had deceived everybody, and would still deceive many. They were therefore rightly called at first False Coal Measures. Geologists will recall how completely they deceived the Nova Scotia geologists, even into inventing an extraordinary theory of lagoons and back deposites. They still exist, and they must have some name. They are still deceptive. What shall we call them?

But Mr. Lesquereux is not even in favor of acknowledging their existence as a separate series. He wishes to include them in the Coal Measures. He says the distinction is invalid, because it is based upon the fact that they are not generally found over the whole extent of the coal-fields of America. I think my friend is wrong. He underrates their extent. And then again the distinction, I take it, is not based upon horizontal considerations but upon vertical ones; based upon the immense interval between the two series as observed along their Alleghany mountain outcrops. He will recollect that at the head of the Juniata up the gorge of Tipton Creek, there are two open gangways in what are probably *two* coal-beds three feet thick, *six hundred feet below the great conglomerate* at the base of the Coal Measures. At Augusta Springs in Virginia, and from Blacksburg and Christiansburg southwestward through Wythe County, Virginia, there are False Coal Measures beneath at least two thousand feet of Red Shale and limestone, and how much more cannot be told because of the vicinity of those immense faults which bring up lower Silurian rocks against the coal. Now these False Coal Measures are surely an independent system; and as such they correspond with a similar independent system in Europe. I believe my friend Mr. Lesquereux has determined their independence also by at least one fossil plant which he has never seen above the Red Shale; that is in connection with the True Coal Measures. This system although to all appearance, full 1500 feet lower in the series than the Tipton coal, cannot be considered other than above it; inasmuch as the Red Shale is only about one hundred feet thick at Tipton, and just beneath the conglomerate. But we may easily imagine that the Tipton coals are the actual representatives of the Virginia system, by considering the thousand feet of Red Shale of the latter to be a lithological enlargement, southward, of the Tipton Red Shale, at the expense of the five hundred feet of Tipton green sandstones which do not appear above the Virginia coals. But there is still another series to be spoken of.

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* See his paper in this Journ., vol. xxviii, pp. 28, 29.

Between the False Coal Measures and the True Coal Measures by which I mean the carboniferous system above a supposed persistent conglomerate stratum, (now however known to be a variable group of sand rocks, shales and coals), there is what might and what would be called a third coal system, if the conglomerate proved to be a universal horizon rock. For underneath the conglomerate, and at the top of the Red Shale, formation XI, (Rogers's Umbral), lie alternations of carboniferous, argillaceous and siliceous shales, containing at least one real coal-bed. This appears everywhere beneath the conglomerate or the sand-rocks at the base of the Coal Measures.

The question for my friend Mr. Lesquereux to settle for us is this:—Whether the coal systems under the conglomerate in eastern Kentucky, as described in my brother's report and shown in his long section, is this sub-conglomerate system of Pennsylvania, and is perhaps also represented by the low Mercer and Trumbull County coals in the Lake Erie district, at Massillon, &c.;—or whether it is the Wythe and Montgomery County sub-red-shale False Coal Measures of Virginia;—or whether it is the reappearance in Kentucky of the Tipton Juniata coal-beds at a still lower horizon;—or whether it *represents all these* in a general thinning out of the Devonian and sub-carboniferous, westward, from the massive outcrops of the Alleghany.

I am sorry that I cannot agree with my friend Mr. Lesquereux when in continuing his argument against the name False Coal Measures he says that "the same [objection that they do not extend over all the coal region] can be said of the coal strata between the Mahoning and the Anvil Rock sandstone, and particularly of the upper Coal Measures above the Anvil Rock." I have already adduced the Somerset County coals to demonstrate the contrary respecting the Pittsburg group of beds (between the Mahoning S.S. and the Anvil Rock); and it seems to me that the contrary is equally clear respecting the group of beds above the Anvil rock; for these beds exist in Green and Washington counties, Pennsylvania, along the Monongahela river, six hundred miles away from the West Kentucky coalfield; and I believe them to exist in the Pottsville Anthracite basin. I sympathize heartily with my friend Mr. Lesquereux in his dislike to the erroneous depths which have been assigned to the Anthracite Measures. But I fear I cannot go all lengths with him in this dislike, when, if I understand his language, he makes the Pottsville basin so shallow as to contain only four nominal coal-beds, or rather the equivalents of the beds from the Kentucky coal No. 4 downwards. This must be a very great mistake, which I think Mr. Shæffer or any other geologist in that region can demonstrate without trouble. If Mr. Rogers has repeated and endorsed the old blunder of separating the Mammoth and Jugular Veins, thereby duplicating the lower part of the measures in the Pottsville basin, and has also given by far too much latitude in his measurements to the faulty condition of the middle of the basin, the incredible thickness of the measures which he thereby obtains should not drive us to the opposite extreme. Now when Mr. Lesquereux says: "from palæontological evidence I am satisfied that the highest coal of the Pottsville and Tamaqua basin is the equivalent of our No. 4, and that the measures do not ascend higher in that part of Pennsylvania," he merely destroys the value of palæontological evidence to the eyes of those of us who have been so long and so lovingly and reverently watching his footsteps on our mountains, bringing us glad tidings of a new science. He certainly forgets that at Shamoken he can count twelve great beds of coal from below upwards, and then can lay his hand upon a conglomerate, which if lithological laws are of any value, must some day prove to be very near his Anvil Rock. He must not forget that even in the shallow synclinals of the Lehigh mountains he has a greater depth of coal than he here assigns to the wide and deep Pottsville and Tamaqua basin. He must not forget that the sharp mountain exhibited to him in every one of its gaps, a row of vertical coal-beds, long enough to more than include the sub-Anvil rock group. It seems as well demonstrated as the absence of fossils will permit it to be, that we have in the middle folds of this great anthracite basin, in either the group of the Salem vein, or of the Peach Mountain, the representatives of the super-anvil-rock coals of western Kentucky, and of the Green and Washington County coals of southwestern Pennsylvania.

In a letter just received (Aug. 20th) Mr. Lesquereux informs me that when last in Pottsville, Mr. Schaffer and other gentlemen, coal proprietors, assured him that the Salem, Gate, and Tunnel veins, which in the face of every opposition he had always identified, were now found to be in fact the same, or belonging to the same group. In that case we must permit him certainly to place them in as low a botanical position as he has. This will not invalidate the series made out from the northern and undisturbed flank of the basin, along the Mine Hill. It will leave, in fact, the basin as deep as any in the United States.

My friend thinks and says, with the enthusiastic confidence of a master in palæontology, which he is,—in fact our great master in the botany of the coal,—that we structuralists can do nothing to help the great questions of East and West to a solution; that "it is only by palæontology that the equivalency of the coal strata has been and can be established in distant parts of the same basin, and especially in separate coal basins." If this be said exclusively of the drift-covered basins of the far west, it is reasonably true; but when stated broadly and made therefore applicable to the whole coal area it is surely unacceptable. Palæontological identification is certainly the crowning glory of our science, the *abendröthe* of our working day, and the reward we receive for patient toil in one department by finding at the far end a door thrown open for us into a new and finer one. But let every tub stand upon its own bottom. Palæontology and Lithology must learn to live and let live. My friend's botanical conclusions are some of them yet to be modified by structural examinations; as in fact they were reached through structural observations. The patient tracing of outcrops from ravine to ravine, and from county to county; the patient construction and comparison of cross sections on hillside after hillside, until not an exposure has escaped; the patient triangulation and levelling of area after area until a perfect map of the surface has been obtained; and the patient underground working up of gangways and breasts, tunnels and shafts, which reveal blunders no surface exhibitions could have corrected:—What is the best palæontological work in comparison with all this—*for results?* What is even the patience, the skill and the genius of Lesquereux, for a life-time, to a combination of apparatus and opportunity so overwhelming and complete? The "comparative sections" in the table were all constructed with the very slightest references possible to palæontological evidence. The Freeport-Curlew limestone and the Ferriferous limestone are great *lithological* horizons, entirely established by lithological and structural geology.

The opinion expressed by my friend Mr. Lesquereux "that coal No. 1, with its members B and C, and perhaps No. 2 subdivides, forming as many as eight different strata," is an evidence how little structural difficulties appall palæontologists. To my mind, filled with the experiences of twenty-five years in coal, such a thing is simply incredible. So far as my observation has covered the ground, no coal-bed is known to separate permanently into two or more unless it is essentially a double or triple bed; that is, unless it evidently consists, at the points where it seems to be but one bed, of two or more, by virtue of persistent interlaminations of fire clay, or some equivalent rock. I have never seen an exception to this rule. The great bed of the lower anthracite coal is a permanently triple bed, and therefore its members are sometimes separated by twenty or thirty feet of slate. The great Mauch Chunk Summit Mine Coal is, even to the uninstructed eye, a group of beds, spreading somewhat further asunder elsewhere. But that any one of the small western bituminous coals should first so separate in parts as to obtain regularly lettered or numbered "members" and then these again split up until the one bed should be represented by a tall cross section containing eight separate beds of coal—I cannot easily believe it.

In conclusion I wish to suggest to geologists who take a special interest in the carboniferous formation, that the term Mahoning sandstone has become as unsafe a name for a horizon line, as is the term Conglomerate. My surveys for the Pennsylvania Railroad Company some years ago, in Indiana, Fayette and Westmoreland Counties, made me aware of several important rocks in the Barren Measures by which that whole group, (and I must still consider it an *interval* separating most decidedly the upper and lower divisions of the true Coal Measures), can be discussed intelligently. One of these rocks is a conglomerate of weight and character, appearing over a considerable country, and forming as I believe, as general a horizon as any eminent sand-rock can. It underlies the Pittsburg coal-bed one or two hundred feet; overlies the red bands of the Barren Measures; forms the coping rock along the centre of the Ligonier valley north of the Conemaugh, and crowns with cliffs the ravines of Castleman's river, in southern Somerset, above the Turkey-foot. For a long time I confounded it with the Mahoning sandstone, the true place of which is from one to three hundred feet further down. But I afterwards learned to distinguish and value it apart.

J. P. Lesley.

Philadelphia, Aug. 1, 1861.

3. *On the origin of some Magnesian and Aluminous Rocks;* by T. Sterry Hunt, F.R.S., of the Geological Survey of Canada. (From the Canadian Naturalist for June, 1860.)—In common with other observers, I have long since called attention to the fact that silicates of lime, magnesia and oxyd of iron are deposited during the evaporation of many natural waters, such as the mineral springs of Varennes and Fitzroy, and the waters of the Ottawa river. I have also suggested that the silicates thus produced may have contributed in a considerable degree to the formation of rocks. (This Journal, March, 1860, p. 284). A hydrous silicate of magnesia which approaches in composition to MgO SiO^3, combined with from ten to twenty per cent of water, and mechanically mixed with small portions of oxyd of iron, alumina and carbonates of lime and magnesia, forms extensive beds with limestones and clays in tertiary strata, in France, Spain, Morocco, Greece and Turkey. It is the sepiolite of Glocker, the meerschaum of some authors, the magnesite of others. The quincite of Berthier, which occurs in red particles disseminated in limestone, is a similar compound, containing some oxyd of iron. The sepiolite from the basin of Paris occurs beneath the gypsiferous group, and in the lacustrine series known as the St. Ouen limestone, where it forms very fissile shaly layers, enclosing nodules of opal (menilite). The structure of this sepiolite, which I have examined and described as above, and that from Morocco, which is used by the Moors in their baths as a substitute for soap, and has been described by Damour, is peculiar. The mineral is made up of thin soft scales, and when moistened with water, swells up into a pasty mass resembling a finely divided talc. Although agreeing closely with this mineral in the proportions of silica and magnesia, sepiolite contains more water, and both before and after ignition is soluble in acids, which talc is not. We cannot however doubt that talc and steatite have been formed from sepiolite, which has undergone a chemical change and become insoluble. It is possible that serpentine may be derived from another silicate richer in magnesia than sepiolite. The frequent association of carbonates of lime and magnesia with talc, and of carbonate of magnesia, talc and serpentine, as in the ophiolite of Roxbury (This Jour., [2], xxv, 224,) would seem opposed to the notion that serpentine may have been formed from the alteration of a mixture of sepiolite and carbonate of magnesia. In chlorite, which often forms rock masses almost without admixture, we have an alumino-magnesian silicate which cannot have been derived from sepiolite, inasmuch as this contains for the amount of magnesia present, twice as much silica as chlorite. The oxygen ratios of the silica and magnesia in sepiolite are as 3 : 1, and those of silica, alumina and magnesia (including the variable amount of ferrous oxyd which in part replaces the latter) in chlorite are as 6 : 3 : 5, while in the purest clays the ratio of silica and alumina equals 1 : 1, and in most argillaceous sediments the proportion of silica is still greater. It is evident, therefore, that chlorite could not be formed from a mixture of sepiolite with clay, or even with pure alumina, without the elimination of a large amount of silica, and we are led to regard it as having been generated by the reaction of a silicate of alumina or clay with magnesia, which was probably present in the unaltered sediment in the form of carbonate. Unless indeed the process, which according to Scheerer, has in recent times caused the deposition from waters, of

neolite, a hydrous alumino-magnesian silicate approaching to chlorite in composition, be the type of a reaction which formerly generated beds of chlorite, in the same way as those of sepiolite or talc.

A silicate of lime allied to sepiolite, has not so far as I am aware, yet been noticed among unaltered sediments, and among crystalline strata calcareous are more rare than magnesian silicates, although double silicates of lime and magnesia (pyroxene and hornblende,) often form beds, and wollastonite, either alone or mingled with carbonate of lime, sometimes constitutes rock masses. The double silicates of alumina and lime are however abundant; the lime-feldspars, scapolite, epidote (saussurite), and white garnet, all form beds in crystalline rocks. Reactions in water at the earth's surface, and at no very elevated temperature, may have given rise to double silicates of lime and alumina corresponding to neolite, and allied in composition to the zeolites, and these by subsequent metamorphism have been changed into anhydrous silicates. The production of harmotome, chabazite and apophyllite by the waters of a spring at Plombières, at temperatures not above 160° F. as observed by Daubrée, lends probability to such a view.

But while we admit the possible direct formation of double silicates in water at ordinary temperatures, there is not wanting evidence that the reaction which we long since pointed out, (Proc. Royal Society of London, May 7, 1857, this Jour., [2], xxv, 287), between silicious and argillaceous matters and earthy carbonates in presence of alkaline solutions, intervenes in the metamorphism of sedimentary rocks and in the production of many silicious minerals. The blue Silurian limestones of the island of Montreal, when treated by acids leave an insoluble residue, which contains about ten per cent of soluble silica, mixed with an argillaceous matter whose analysis gave silica 73·0, alumina 18·3, potash 5·5, and only traces of lime and magnesia. In the vicinity of an intrusive dolerite, however, the limestone is changed in color, and leaves by the action of acids a greenish matter, which consists of silica 40·2, alumina 9·3, peroxyd of iron 5·2, lime 36·6, magnesia 3·7. The free silica and that of the intermingled aluminous silicate, has thus been saturated with protoxyd bases, still, however, retaining the alumina in combination. A similar reaction with more aluminous matters, would give rise to epidote, garnet, magnesian mica, scapolite or feldspars like labradorite and anorthite, and it is not impossible that in such reactions a portion of alumina may sometimes be set free, and give rise to corundum, spinel, diaspore or völknerite.

In the ordinary modes of decomposition of minerals containing alumina, this base separates in the form of silicate, and the conditions required for its elimination in a free state are but imperfectly understood. We have elsewhere pointed out the decomposition by alkaline and earthy carbonates, of solutions of sulphate of alumina or native alum, as one source of free alumina, and insisted upon the existence of pigotite, a native compound of alumina with an organic acid, as an evidence that this base is sometimes like oxyd of iron, and oxyd of manganese, (this Jour., [2], xiii, 12), taken into solution by water aided by organic matters. A hydrate of alumina, gibbsite, is found associated with limonite, and the aluminous minerals from the south of France described by Berthier and Deville, show that free alumina is much more common in nature than was formerly supposed. Berthier long since gave the name of bauxite to an earthy pisolitic ore which occurs either massive or imbedded in limestones of tertiary age, at Baux, and many other localities in the departments of Gard and Var, and also in Calabria, and the Grecian Archipelago, forming in some places an abundant rock.* This substance is a variable mixture of a hydrate of alumina, apparently approaching diaspore in composition, with hydrous peroxyd of iron, sometimes constituting a workable iron ore, and at other times a veritable ore of alumina. It contains besides small portions of silica, titanic, vanadic and phosphoric acids, and occasionally encloses grains of corundum. A compact dark red variety gave Deville, alumina 57·6, peroxyd of iron 25·3, and water 10·8, besides 3·1 of titanic acid, and 2·8 of silica. In other specimens the proportions of alumina and iron oxyd are nearly equal, or the latter predominates, as in one example where the proportions were 48·8 of iron oxyd, and 32·2 cf alumina; and another, 60 of iron, and 18 of alumina and titanium. In these analyses the carbonate of lime, generally present, was first removed by a dilute acid; the prolonged action of stronger acids completely dissolves the hydrated oxyds. By an intense heat this substance is converted into crystalline corundum, resembling emery in its physical character, but the presence of grains of corundum in the hydrated mineral seems to show that this transformation may take place at ordinary temperatures. The emery of Greece and Asia Minor, which is associated with variable proportions of oxyd of iron, is according to Dr. J. Lawrence Smith always more or less hydrated.

The argillaceous matter enclosing some varieties of this bauxite or impure diaspore, is white, without plasticity, and very rich in alumina; one specimen freed from the red ferruginous portions, gave alumina 58·1, silica 21·7, peroxyd of iron 3·0, titanium 3·2, water 14·0. This substance approaches in its composition to collyrite, and to the dillnite which is the gangue of the diaspore of Schemnitz. These materials however contain from 20 to 40 per cent of water. Scarbroite, schrötterite, and allophane are similar matters; the latter, unlike a clay in its structure, appears to have been deposited from solution. The subsulphate of alumina, known as websterite or aluminite, is often met with in layers and concretionary masses in Tertiary clays,† and is sometimes mingled with a silicate having the composition of allophane. This frequent occurrence of alumina still retaining a portion of sulphuric acid, confirms the view which we have elsewhere expressed, that solutions of native alums have by their decomposition furnished the alumina for many of the minerals in question, while the conditions under which this base is taken into solution by organic matters, still require investigation. The careful examination of unaltered sedimentary deposits, is calculated to throw great additional light upon the origin of the crystalline rocks.

4. *Note to the Paper of Messrs. Meek and Worthen on the Age of the Goniatite Limestone*, p. 167 of this No.—The name "Kinderhook Group" is now proposed by these authors to include the beds lying between the Black Slate and the Burlington Limestone, which have heretofore been considered the equivalents of the Chemung Group of New York. This designation will be observed in the Illinois Report.—(*Letter to Editors.*)

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* Deville, *Ann. de Chimie et Physique*, (3) lxi, 309.

† In this connection we may notice apatelite, a basic persulphate of iron, which occurs in conditions similar to aluminite.

III. BOTANY.

1. *Martius, Flora Brasiliensis.*—Since our last notice, two more issues of this great work have come to hand: viz., fascicles 25 and 26, containing the *Santalaceæ* (two species of *Thesium*) and *Myristicaceæ* (26 species of *Myristica*) by Alph. DeCandolle, and the more numerous *Apocynaceæ*, which are thoroughly elaborated, with decided talent, by DeCandolle's curator, J. Müller, from whom excellent work may be expected, judging from this specimen. This was published in July, 1860. It is now followed by fascicles 27 and 28, published in February last. This contains the *Antidesmeæ* by Tulasne, consisting merely of three species of *Hieronymia* (the *Stilaginella* of Tulasne), the representatives of *Antidesma* in the new world, the group pretty clearly belonging to *Euphorbiaceæ;* also the *Begoniaceæ* by DeCandolle (83 species of *Begonia*), followed by a few pages of interesting disquisition by Von Martius himself. Finally we have the *Celastrineæ*, *Ilicineæ*, and *Rhamneæ*, by S. Reissek of Vienna, copiously illustrated, and with the leaves of many species self-printed by the Vienna process. The elaboration of these orders appears to be very satisfactory. We congratulate Von Martius upon the character and steady progress of this formidable work. A. G.

2. *Floræ Columbiæ terrarumque adjacentium Specimina Selecta*, edidit H. Karsten, Tom. I, fasciculus primus. Berol: F. Dümler, 1858. pp. 42, tab. 1–20, col. imp., fol.—This promising commencement of a magnificent work has just reached us, with an advertisement by the successors of the original publisher, dated Feb. 1859, announcing that the publication, if the subscription warrants, will extend to ten such fasciculi, issued at intervals of six months; the price of each, with colored plates, twenty Prussian thalers; the uncolored copies, fifteen thalers. We trust the needful patronage will not be wanting. The plates are very fine, and with satisfactory analyses; and are accompanied each by two pages of letter-press; the characters in Latin; the detailed descriptions and remarks in the German language. The subjects are well chosen; the first representing two new Palms, species of Klopstockia from the Quindiu Andes; the second a Tree-Fern, *Cyathea ebenina* from near Caracas; eight others representing species of *Cinchona* or near relatives of this most important genus, and three others illustrate Rubiaceous genera, *Poggendorffia rosea* seems to be a good *Tacsonia*. The pretty Cobæaceous *Rosenbergia penduliflora*, t. 14, with long ligulate lobes to the corolla and extremely long filaments, is in Fendler's Venezuelian collection, No. 468.

A. G.

3. *Journal of the Proceedings of the Linnæan Society*, (Botany), No. 20, (1861) finishes Hooker and Thompson's precursory account of the Indian *Cruciferæ*, and contains five other papers; four of them very short ones, and of only local interest. Dr. Welwitsch's letter on the botany of Benguela, Western Africa, gives an account of a very queer dwarf tree, found near Cape Negro, the stem of which, "with a diameter often of four feet, never rose higher above the surface than one foot, and which through its entire duration, that not unfrequently might exceed a century, always retained the two woody leaves which it threw up at the time of germination, and besides these it never puts forth any other.

The entire plant looks like a round table, a foot high, projecting over the tolerably hard, sandy soil, the two opposite leaves, (often a fathom long, by 2 to 2½ feet broad) extend on the soil to its margin, each of them split up into numerous ribbon-like segments. The flowers are hermaphrodite and collected in a sort of aments or strobiles." There has also appeared a *Second Supplement* to the botanical portion of vol. v, (1861) which contains four very interesting papers, viz:

(1.) The Natural Order *Aurantiaceæ*, with a Synopsis of the Indian Species, by Daniel Oliver, the newly appointed Professor of Botany in University College, London, who here takes rank as a first-rate monographer.

(2.) Notes on *Menispermaceæ*, by George Bentham, who now, as botanists will be pleased to learn, occupies the presidential chair of Brown and Smith in the Linnæan Society. That the so-called opposition of the parts of the flower in *Menispermaceæ* and *Berberidaceæ* is only in seeming, must we suppose be every where understood and admitted. As to the opposition of stamens to petals in *Rhamneæ* and the like, if shut up to the alternatives mentioned by Mr. Bentham, we should still prefer "the idea of a common origin" to that of the stamen being developed in the axil of the petal, which would introduce a needless and most improbable complexity into the morphology of the flower. But a third alternative, which was long overlooked, appears to offer a more natural explanation, conformable with the laws of the arrangement of leaves, viz., that the stamens are here normally superposed just as the parts of the successive cycles of alternate leaves are,—the ordinary alternation of floral parts being really the thing to be accounted for.

(3.) Notes on *Tiliaceæ*, and 4, Notes on *Bixaceæ* and *Samydaceæ*, by the same author, are masterly papers. At the first view we were far from satisfied throughout with the proposed re-arrangements, but the more we consider the matter the better we like it, even in the association of *Cochlospermum* and *Amoreuxia* with *Bixa.* We should still prefer the union of the *Sanydaceæ* with the *Bixaceæ*, and make less than Mr. Bentham does of the difference between hypogynous and perigynous insertion,—a difference often obscure or shading away, and as often coëxisting in the same natural group, or among closely related ones, as parietal and axile placentation do. No one knows this better than the sagacious authors of the forthcoming new Genera Plantarum; but their adhesion to the old tradition is compelled by their determination "to maintain the Candollian series in its general features,"—which is certainly still desirable; but if rigidly followed out in this respect will certainly do much violence to natural affinities, perhaps without securing adequate practical advantages. But it is far easier to interpose objections than to collocate the natural orders upon any consistent system. A. G.

4. *Calluna vulgaris and Aira caryophyllea in the United States.*—That "America has no Heaths" is a botanical aphorism. It is understood, however, that an English surveyor nearly thirty years ago found *Calluna vulgaris* in the interior of Newfoundland. Also that De la Pylaie, still earlier, enumerates it as an inhabitant of that island. But this summer, Mr. Jackson Dawson, a young gardener, has brought us specimens and living plants (both flowering stocks and young seedlings)

from Tewksbury, Massachusetts, where the plant occurs rather abundantly over about half an acre of rather boggy ground, along with *Andromeda calyculata*, *Azalea viscosa*, *Kalmia angustifolia*, *Gratiola aurea*, &c., apparently as much at home as any of these. The station is about half a mile from the State Almshouse. Certainly this is as unlikely a plant, and as unlikely a place for it to have been introduced by man either designedly or accidentally, as can well be imagined. From the age of plants it must have been there for at least a dozen years; indeed, it had been noticed and recognized, two years ago, by a Scotch farmer of the vicinity, well pleased to place his foot once more upon his native heather. So that even in New England he may say, if he will—as a friend of ours botanically renders the lines—that

> " *Calluna vulgaris* this night shall be my bed,
> And *Pteris aquilina* the curtain round my head."

It may have been introduced, unlikely as it seems, or we may have to rank this Heath with *Scolopendrium officinarum*, *Subularia aquatica*, and *Marsilea quadrifolia* as species of the Old World so sparingly represented in the New, that they are known only at single stations,—perhaps late-lingerers rather than new-comers.

Aira caryophyllea has just been detected by Wm. M. Canby, Esq., in New Castle county, Delaware, growing very abundantly, "in a dry piece of ground, rather open, but dotted over with pine trees (*Pinus inops*), and completely surrounded by a forest. It certainly has not been ploughed for ten years, probably not for a much longer time. In company with it was *Polygala Nuttallii*, *Sorghum nutans*, &c., but no clover, timothy, or any of the grasses usually cultivated. Still we suppose this species to have been introduced. A. G.

IV. ASTRONOMY AND METEOROLOGY.

1. *The recently discovered Asteroids.*—In the last No. of the Journal we gave a list of the asteroids discovered since 1858, together with the elements of several of them. We now give the elements of the remainder, and also corrected elements of Titania:

Berlin mean time.	(61) Titania, 1861, Jan. 0.2512.	(67) Asia, 1861, April 21.0.	(68) Leto, 1861, June 1.0.
L	38° 8′ 17″·29	241° 22′ 41″·4	240° 24′ 9″·1
π	98 28 35 ·33	294 23 27 ·4	346 15 37 ·5
☊	191 57 53 ·02	202 0 6 ·5	44 49 43 ·5
i	3 34 21 ·46	5 48 36 ·9	7 58 19 ·8
e	0·1847205	0·144085	0·185692
μ	958″·43568	958″·418	767″ 637
a	2·393109	2·39314	2·77481

Berlin mean time.	(69) Hesperia, 1861, May 13.4864.	(70) Panopea, 1861, June 0.0.
L	160° 52′ 5″·0	253° 11′ 36″·7
π	117 14 2 ·3	299 3 0 ·2
☊	186 52 56 ·0	48 21 0 ·3
i	8 28 55 ·7	11 14 37 ·1
e	0.1779006	0.223545
μ	661″·260	813″222
a	3·064905	2·670127

In the above tables,

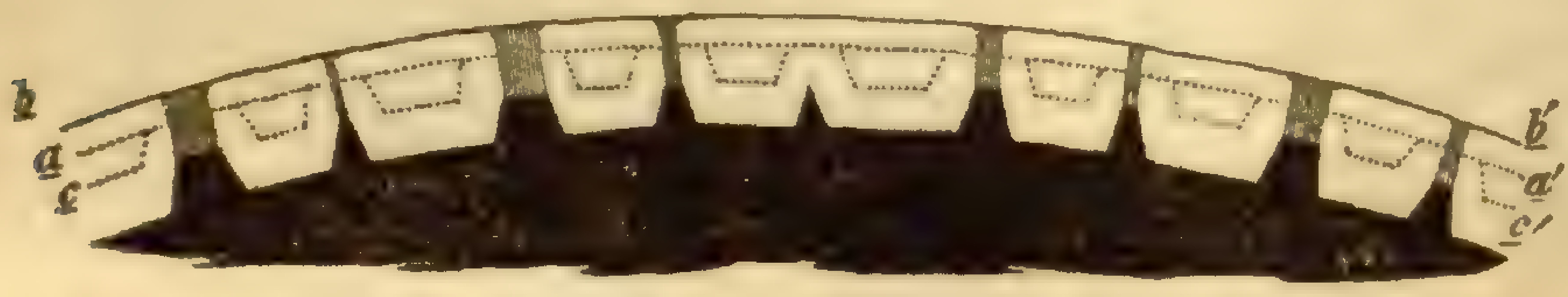
L represents the mean longitude of the planet at Epoch.
π " the longitude of the perihelion.
☊ " the longitude of the ascending node.
i " the inclination of the orbit to the ecliptic.
e " the eccentricity of the orbit.
μ " the mean daily motion.
a " the semi-major axis of the orbit.

2. *Note Explanatory of Baily's Beads;* by EDMUND BLUNT.—We have received from Mr. Blunt the following satisfactory illustration of the cause of the formation of beads and ruptures of the ring, etc., in an annular eclipse of the sun. Although privately circulated, this explanation of Baily's Beads has not before been published.—*Eds.*

Brooklyn, N. Y., May 18th, 1854.

Mr. F. Baily concludes his paper, on the Annular Eclipse, May 15, 1836, thus, "That mountains may occasionally obtrude themselves to our view, there can be no question; but it requires further explanation why almost all of the remarkable distortions take place when observed near the edge of the Sun's disc, and as the Moon advances thereon, these prodigious elevations, in a great measure, disappear." The following is an explanation: (Fig. 1.)

1.



Let the line *a a′* represent the true edge of the Sun's disc, and the exterior line *b b′* the apparent edge, (the difference between the two being caused by the tremulous motion in our own atmosphere,) the zig-zag line *c c′* the true edge of the Moon, and the dark the apparent edge, when seen on the Sun—the difference between the two being caused by the same.

Now, as the true edge of the Sun's disc touches any of the mountains on the Moon's edge, if the part in contact is *more* than twice this difference, dark lines will instantly shoot out and connect the apparent edges of the Sun and Moon, and they become wider as the Moon's edge approaches the exterior edge of the Sun's disc, beads being formed between, or narrower, as the Moon's edge recedes internally, until they break. Fig. 2 shows Baily's Beads at the instant of formation.

2.

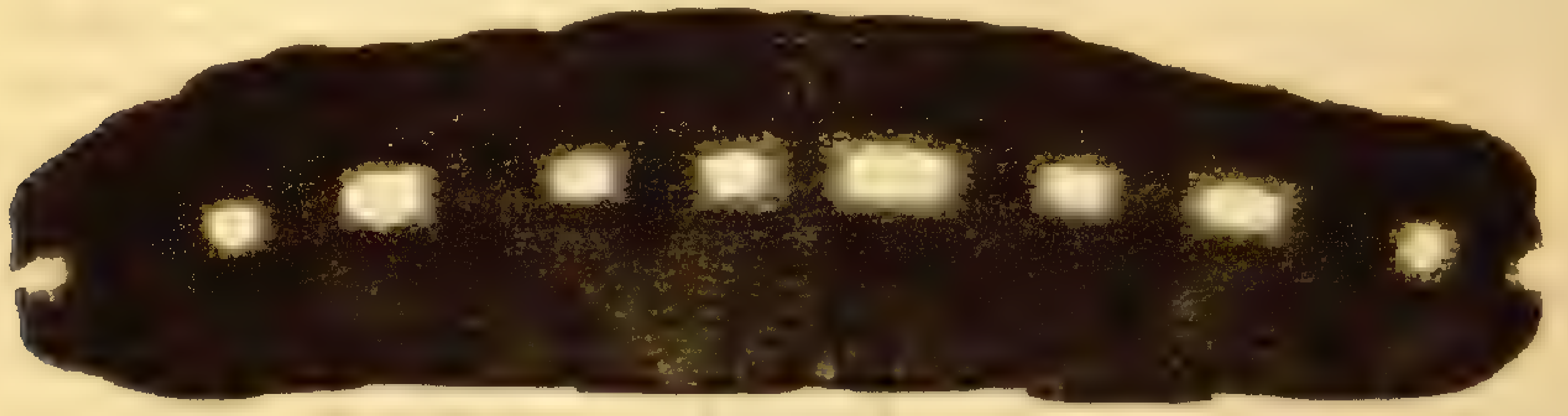
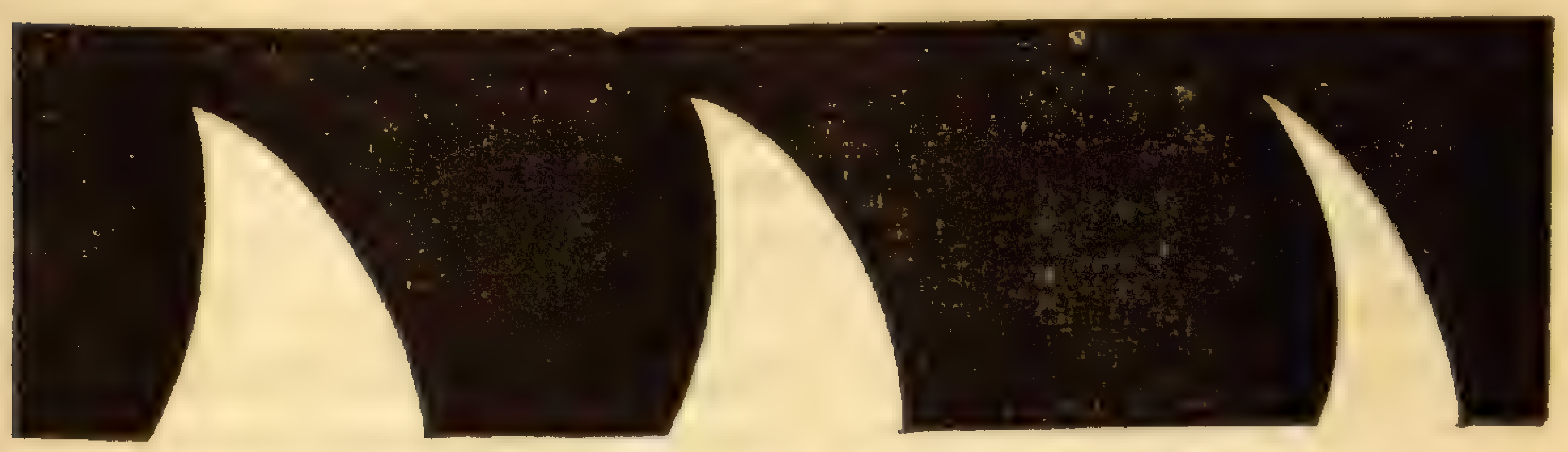


Fig. 3 shows the Sun's lower cusp as seen through an inverting telescope at New York, May 26, 1854.

3.



The following reasons, for the truth of this theory, I beg you to look at:

1st. The observed diameter of the Sun is always greater than the computed. See observations at various observatories.

2d. The diameter of objects projected on the Sun's disc are less than the computed diameters.

3d. Results, obtained from observations at the beginning and end of an eclipse are good; because the contact or end is not seen, except when the Moon's edge touches the true edge of the Sun's disc.

4th. Results obtained by the measurement of the distance of the solar cusps, &c., are not good, and, I think, it is entirely owing to their not correcting the angle, for increase caused by tremulous motion in our own atmosphere, and which amount might be estimated, or even measured during the eclipse, by each observer.

April 13th, 1860.

In addition to the above, I would remark that the annular eclipse, as observed September, 1838, in Brooklyn, showed the truth of this theory. The slow tremulous motion of the atmosphere causing the lines and beads to assume wavy shapes, the Sun's altitude, at the formation of the ring, being low. When the tremulous motion of the atmosphere is so rapid that the wavy lines do not appear, the shooting out of the black lines is almost instantaneous.

The truth of this theory, I tested in the following manner:—At the distance of about eleven miles, was placed a black-board, six feet by eight inches, with a hole in it, two-tenths of an inch in diameter, for the Sun's rays to be reflected through, towards me, with a heliotrope. Towards noon, when the atmosphere was tremulous, a large bright light only was seen, and, as the atmosphere became more steady, the size decreased, until the board began to show as a large faint shadow, which increased in intensity as it decreased in size, and at last showed the board well defined, with a bright point in the centre. The A. M. observations showed the same, but in reverse order.

The eclipse of July 17th (1860) will give an opportunity for observing these distortions, but it will be useless to look for them should there be any moisture, or even a slight mist in the atmosphere, extending over the earth's surface near the observer, as the Sun's diameter at such times, is not increased.

3. *Meteoric Observations, April* 20, 1861.—The following observations at New Haven show no recurrence of the meteoric shower of April 20, 1803, but they are too limited to be satisfactory. The shower may perhaps continue only a few hours, and thus be easily lost even if observers were watching throughout the globe.

Saturday, April 20, 1861. Observers: Mr. Edward R. Sill and myself. Sky clear; moon down. Different meteors observed:

$2^h\ 45^m$ to 3^h A. M.	in N. 4	S. 6
3^h " 4^h "	" 13	" 29

Of these about ten equalled in brilliancy stars of the first magnitude, but none were very remarkable. The majority appeared to come from near the zenith, about the constellation *Lyra*, but they did not show any decided radiant.

On the Monday morning previous, watching alone from $3^h\ 15^m$ to $4^h\ 15^m$, I saw only three or four shooting stars. E. C. HERRICK.

4. *Meteoric Observations, August* 10, 1861.—(1.) *New Haven, Conn.*—At this place, the sky was clouded during the nights of Thursday and Friday, Aug. 8 and 9, 1861. The night of Saturday, the 10th, was clear and favorable. From the top of the tower of the Alumni Building, the following observations were made that night by a corps of four observers, viz. Messrs. Charles Tomlinson, Henry P. Johnston, Willabe Haskell, and myself. We were so stationed as to see as many as possible of the meteors, and yet some were undoubtedly lost. None were reckoned twice.

	N.W.	N.E.	S.E.	S.W.	
Aug. 10–11, 10^h to 11^h P. M.	22	26	16	12	= 76
11^h to midnight,	23	33	21	17	94
0^h to 1^h A. M.	23	38	29	29	119
	Total seen in 3 hours,				289

Of these shooting stars the large majority moved in paths which traced back would intersect near the sword-handle of Perseus. Many equalled or surpassed in brightness stars of the first magnitude, and one at $11^h\ 30^m$, near the zenith, was much more splendid than Venus and left a train of sparks which remained luminous for twenty seconds after the meteor disappeared. This grand meteor was also seen at Providence, R. I., and was fortunately so well observed by Mr. B. V. Marsh at Burlington, N. J., that it will probably be practicable to ascertain its elevation, length of path and velocity.

Prof. A. C. Twining, of this city, observed here independently Aug. 10th and Aug. 11th, and will probably publish his results.

Watching alone from $8^h\ 30^m$ to 10^h P. M. of the 10th, I saw about 20 shooting stars, several of which were brilliant and accompanied by trains. There was, as heretofore noticed, a gradual increase of numbers from evening to morning.

(2.) *Burlington, N. J.*—Observers: Messrs. Benj. V. Marsh and Samuel J. Gummere. The observers, one looking S.W. and the other N.E., were stationed on the top of a house, but the view was somewhat impaired by obstacles. The sky was nearly clear until after midnight, when clouds began to appear in the South and Southeast. After 2^h they caused serious embarrassment, and by 2^h 45^m the sky was so nearly overcast that the observers retired. Different meteors observed:

1861.	N.E.	S.W.	
Aug. 10–11, 10^h 25^m to 11^h P. M.	20	20	= 40
11 " 12 "	29	29	58
12 P. M. " 1 A. M.	47	38	85
1 A. M. " 2 "	35	29	64
2 " " 2 45 "	28	14	42
			289

Mr. Marsh, who was looking towards the N.E., writes: "At first I could not fix the radiant definitely, most of the tracks pointing to Perseus, but a number to Cassiopeia. Towards midnight it seemed to be very clearly within the triangle formed by η, γ, and τ Persei. I could not detect any change of position afterwards. We thought that seven-eighths of all we saw during the night were conformable. * * * Much the finest meteor seen was at 11^h 23^m,—a most splendid specimen; its track remained visible twenty seconds or more."

Mr. R. M. Gummere, also at Burlington, N. J., watching independently, observed shooting stars as follows, viz:

Aug. 10, 10^h 25^m to 11^h P. M.	34
11 " 12 "	29
12 " 1 A. M.	37

The night of Aug. 9–10 was stormy at Burlington.

(3.) *Natick, Mass.*—Observers: Messrs. F. W. Russell and E. L. Pray. The night of August 9th was cloudy and rainy. The night of the 10th was clear until about 2 A. M. of the 11th, when about a third of the sky was obscured by clouds. Between 8^h P. M. and 3^h 15^m A. M., the two observers saw 397 different shooting stars, as follows:

Aug. 10–11.	N.E.	S.E.	N.W.	S.W.	
8^h P. M. to 9^h P. M.	16	5	5	3	= 29
9 " " 10 "	10	2	2	8	22
10 " " 11 "	32	6	5	17	60
11 " " 12 "	28	13	3	30	74
12 P. M. " 1 A. M.	27	12	7	13	59
1 A. M. " 2 "	26	14	27	8	75
2 " " 3 "	14	27	12	8	61
3 " " 3 15 "	4	4	1	8	17
					397

During this period only about one-half the sky was under observation; and from 2 to 3 A. M. not more than half the sky was clear. Only four very large ones were noticed, twenty about the size of Jupiter, one leaving a luminous train of smoke for eight seconds, and the remainder quite small. The radiant, to which two-thirds of the meteors conformed, was a spot about three degrees in diameter, around *beta Persei.*

The foregoing observations show that the meteoric sprinkle of August occurred this year (1861), with the usual characteristics. The number may have been somewhat diminished, but was still very much above the average of ordinary nights. This average is greater than is usually supposed, and the actual number varies greatly on different nights. On the night of July 3, 1861, four observers here saw in the hour ending at 10^h 5^m P. M. *thirty-six* shooting stars, or about half as many as are seen during the corresponding hour on the 10th of August.

It is to be hoped that observers, situated from 50 to 100 miles apart, will hereafter act in concert on the nights of meteoric abundance, so that in case of meteors remarkable for magnitude or other peculiarity, data may be obtained for determining their astronomical elements. E. C. H.

5. *Remarkable Rain-Fall in Ohio.*—(Extract of a letter from S. B. McMillan, dated East Fairfield, O., Aug. 15, 1861, to the Editors). "The same day (Aug. 12th) witnessed the greatest fall of rain ever recorded in this part of the State, in an equal time.

From noon	to 1^h P. M.	there fell	0·32	in.	rain.
" 4^h 30^m	" to 5^h 30 P. M.	"	1·40	"	"
" 6^h 30	" 9^h "	"	2·85	"	"
" 9^h	" 11^h "	"	3·44	"	"
Total,			8·01	"	" in 11 hours;

6·29 *inches of which fell in four and a half hours.*

This maximum rain-fall existed over at least 100 square miles. Few dams or bridges are left standing along the streams in this county (Columbiana)—many sheep, cows and horses were drowned, four or five dwelling houses were swept to destruction. The dead bodies of four persons of respectability, and the only persons known to have been drowned, were found the second day after the rain several miles below where the dwelling house in which they were swept away was crushed in pieces. In the village from which it was taken the water was seven feet higher than ever before known, reaching to the second story of nearly every house in the town. There was almost no wind here and only a moderate amount of lightning. A heavy wind is reported 30 miles east of here and also at Cleveland and Sandusky, where it did much damage."

6. *Rain following the discharge of Ordnance*; (note to the National Intelligencer, July 25, 1861.)—Messrs. Editors: In October, 1825, I took note of a very copious rain that immediately followed the discharge of ordnance during the celebration of the meeting of the waters of Lake Erie and the Hudson, upon the completion of the Erie canal; and in 1841 I published my continued observations on the subject, which, to my mind, fully established the fact that the discharge of heavy artillery at contiguous points produces such a concussion that the vapor collects and falls generally in unusual quantities the same day or the day following.

The early battles of the late war between the French, Sardinians, and Austrians were succeeded by such copious rains that even small rivers were not fordable; and during the great battle of Solferino a storm arose of such fierceness that for the time the conflict ceased. Within the last two weeks McClellan's columns on the upper Potomac fought four different battles on as many days, and there were extensive rains before the close of each day. July 21st the great battle of Bull Run, Virginia, was fought, and next day (22d) the rain was copious all day and far into the night. * * * * J. C. Lewis.

A more exact collection of such data as Mr. Lewis has here grouped may lead to important results equally in theory and practice.—Eds.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Earthquake on the Island of Penang.* (In a letter to the Editors, dated U. S. Naval Observatory, Washington, 24th August, 1861.)—*Gentlemen*:—Mr. Geo. E. Tyler who has just returned from the East Indies, communicates to me the following:—

"*Saturday, Feb.* 16, 1861.—At 7½ P. M. there was a severe earthquake shock of sufficient force to throw down crockery from shelves, stop clocks, and to set articles to swinging. In walking, the ground seemed to undulate sufficiently to make many persons feel nausea. There appeared to be three distinct shocks, each of which seemed to pass from North to South and to continue 30 seconds.

"*March 2d.*—I have learned since the above, that the shock was felt at Malacca and Singapore at the same time at each place and with about the same degree of violence, though it was not recognized by many vessels in the harbors and on the passage here from Singapore.

"About five minutes before the shock, there was an unusual commotion in the sea and I spoke of it to others at the time. There was no perceptible air stirring, and the sky was clear. Thermometer 91° Fahr. No other earthquake has been known here during the last 17 years and then only a very slight one."

Very respectfully yours,
J. M. Gilliss, *Supt.*

2. *Earthquake at Syracuse, New York.*—The Syracuse Standard of July 12th says: We learn from various sources that a very sensible shock of an earthquake was felt in this city and other parts of the county last evening, about nine o'clock. The weather yesterday very suddenly became quite cold and chilly, an extraordinary change from the intense heat of the previous days. The shock was about four seconds in duration, and was so severe as to cause dwelling-houses to rock, and in some cases furniture was removed and persons sitting in chairs were waved to and fro. Many persons supposed some of the fixtures of their dwellings had fallen upon the floors. A gentleman from the north part of the town of Salina informs us that the shock was sensibly felt in that section, and farmers ran out of doors supposing that their barns or outhouses had fallen.

3. *On the Natural Dissemination of Gold.*—Mr. Eckfeldt the Principal Assayer of the U. S. Mint at Philadelphia, has lately made several interesting examinations tending to show the very wide distribution of gold. Passing over the evidence respecting its presence in various galenas, in metallic lead, copper, silver, antimony, &c., we cite the following:—perhaps the most curious result of all.

Underneath the paved city of Philadelphia there lies a deposit of clay, whose area, by a probable estimate, would measure over three miles square, enabling us to figure out the convenient sum of ten square miles. The average depth is believed to be not less than fifteen feet. The inquiry was started whether gold was diffused in this earthy bed. From a central locality, which might afford a fair assay for the whole, the cellar of the new market-house in Market Street near Eleventh Street, we dug out some of the clay at a depth of fourteen feet, where it could not have been an artificial deposit. The weight of 130 grammes was dried and duly treated, and yielded one-eighth of a milligramme of gold; a very decided quantity on a fine assay balance.

It was afterwards ascertained that the clay in its natural moisture loses about fifteen per cent by drying. So that, as it lies in the ground, the clay contains one part gold in 1,224,000.

This experiment was repeated upon clay taken from a brickyard in the suburbs of the city, with nearly the same result.

In order to calculate with some accuracy the value of this body of wealth, we cut out blocks of the clay, and found that on an average, a cubic foot, as it lies in the ground, weighs 120 pounds, as near as may be; making the specific gravity 1·92. The assay gives seven-tenths of a grain, say three cents' worth of gold to the cubic foot. Assuming the data already given, we get 4180 millions of cubic feet of clay under our streets and houses, in which securely lies 126 millions of dollars. And if, as is pretty certain, the corporate limits of the city would afford eight times this bulk of clay, we have more gold than has yet been brought, according to the statistics, from California and Australia.

It is also apparent that every time a cartload of clay is hauled out of a cellar, enough gold goes with it to pay for the carting. And if the bricks which front our houses could have brought to their surface, in the form of gold-leaf, the amount of gold which they contain, we should have the glittering show of two square inches on every brick.—*Am. Phil. Society Proceed.*, viii, 273.

4. *Notice of Chemical and Physical Apparatus, by Becker & Sons.*—The Sheffield [Yale] Scientific School has recently procured from Becker & Sons, 54 Columbia st., Brooklyn, N. Y., some instruments of such excellent device and workmanship that we deem it a duty to the scientific community to call attention to them. 1st, An air-pump for laboratory use. This has a single cylinder, the piston of which is solid and has a diameter of 3¼ inches and a seven inch stroke. The communication between the interior of the cylinder and the receiver is opened and closed by means of a two-way cock which is operated by the motion of the pump-handle. The instrument is compact, light, of simple construction but most accurate workanship, and from the absence of valves is *always ready* to furnish a good vacuum with a few minutes labor. It serves also as a condenser, and is adapted for transferring gases. It is the best instrument we have ever seen for the chemical laboratory. It cost $100.

2d. The Balances of Becker & Son, we are also prepared, after considerable experience in the use of those of Oertling, Deleuil and Hugershoff, and after having seen the instruments of nearly every balance-maker of this country and Europe, to pronounce the best known to us. They combine accuracy and delicacy with convenience and cheapness, to a higher degree than any Balances we have hitherto met with. There are now in the Analytical Laboratory here, three of the Becker Balances, viz., an assay balance, which, loaded with 5 grms. in either scale readily indicates $\frac{1}{50}$ of a milligramme, an Analytical Balance carrying 200 grms. in each pan readily indicating $\frac{1}{10}$th miligramme and a Physical Balance which with a load 5 kilogrammes is turned by one centigramme. See advertisement.

3d, A standard and mountain siphon Barometer with inch and millimeter graduations belonging to the Laboratory is a very beautiful and precise instrument.

The inch employed by Becker & Sons is derived directly from pendulum measurements of their own.

4th, finally, the Engineering Dept. has a Theodolite adapted also as a transit, of most perfect device and construction.

The self-recording meteorological instruments and beautiful aneroids which Becker & Sons have in operation at their *atelier*, are worthy the notice of all physicists.
S. W. J.

Book Notices.—

1. *The Mathematical Works of Isaac Barrow, D.D., Master of Trinity College, Cambridge.* Edited for Trinity College by W. Whewell, D.D., Master of the College. Cambridge, 1860. 754 pages, 8vo.—This volume contains the lectures which Dr. Barrow delivered as Lucasian Professor of Mathematics at Cambridge; an office which he held from 1664 to 1670. These lectures consist of three series: the Lectiones Mathematicae, the Lectiones Opticae, and the Lectiones Geometricae; the first being on the general principles of mathematics, the second containing propositions of Optics proved geometrically, and the third treating of properties of Curve lines. The mathematical lectures are twenty-three in number, and were delivered in 1664, 5 and 6, but were not published till 1685.

After these, are given four lectures in which the author proposes to expound the method by which Archimedes invented his theorems concerning Cones and Spheres.

In the Lectiones Opticae, eighteen in number, Dr. Barrow treated of the theory of the foci of spherical surfaces and lenses; and explained the cause of the rainbow, simplifying Cartesius' calculations.

The Sectiones Geometricae, thirteen in number, first published in 1670, are full of curious methods of determining the areas and tangents of curves, many of which are very close anticipations of Newton's methods. The most noted of these is the method of drawing tangents to curves given in Lect. X, Art. 14. This method is justly held to be an anticipation of the Differential Calculus, and to approach very near to it.

Barrow's rule for finding the subtangent of a curve is this: "After constituting the equation to the curve, put $x-a$ and $y-e$ for the ordinates x and y; expand and reject all the terms in which there is no a or e; (for they destroy each other by the nature of the curve;) reject all the terms in which a or e are above the first power, or are multiplied together; (for they are of no value compared with the rest, as being infinitely small;) then put y for a and t the subtangent for e; and the subtangent is found." Barrow applies this method to determine the subtangents of several curves.

In 1669, Newton showed to Dr. Barrow some papers in which the method of fluxions was faintly indicated, and rules given for the rectification and quadrature of curves, and for drawing tangents to curves; and it appears that Newton had been in possession of this method from the year 1666.

In the year 1672, the celebrated Leibnitz communicated to some members of the Royal Society, certain researches relating to the differences of numbers, and in 1674 he announced that he possessed important theorems relative to the quadrature of the circle by series.

In 1676 Newton sent a letter to Oldenburg, which was to be shown to

Leibnitz, in which he describes the properties of his method of fluxions, as well for the determination of tangents, as the quadrature of curves. In 1677 Leibnitz sent to Oldenburg, to be communicated to Newton, a letter containing an account of his *Differential Calculus.* In 1684 Leibnitz published his new method in the Leipsic Acts; but Newton's method of Fluxions did not become generally known till the publication of the Principia in 1686. Leibnitz is admitted to have been the first to publish to the world the principles of the Differential Calculus; but Newton was unquestionably acquainted with the leading principles of the calculus before they were invented by Leibnitz; and Dr. Barrow had a method of drawing tangents to curves very similar to that furnished by the calculus, before either Newton or Leibnitz had devoted any special attention to the subject.

2. *Results of a scientific mission to India and High Asia undertaken between the years* 1854 *and* 1858, *by order of the court of Directors of the Honorable East India Company, by* HERMANN, ADOLPHE and ROBERT DE SCHLAGINTWEIT. With an atlas of panoramas, views and maps. Volume I. London: Trübner and Co. 1861. 512 pages, large quarto.—This volume contains the astronomical determinations of latitudes and longitudes and the magnetic observations made during the scientific journey of the brothers De Schlagintweit through India and High Asia. One of the chief objects of this journey was the completion of the magnetic survey of India, which had been commenced in 1846, by the late Captain Elliot in the Eastern Archipelago; but in consequence of the high interest evinced in it by the Directors of the East India company, the mission assumed a very general and extensive character. The brothers De Schlagintweit left England Sept. 20, 1854, and arrived in Bombay Oct. 26th. One of the brothers, Adolphe, was killed at Kashgar, in August, 1857;* the other two brothers arrived at Trieste, June 8, 1857. The results of this mission are to be embraced in nine quarto volumes of which the following are the titles:

I. Astronomical and magnetic observations.

II. Hypsometry, Barometrical and Trigonometrical observations.

III. Topical Geography, and Route Book of the Himalaya and Tibet.

IV and V. Meteorology and climate in general.

VI. Geology.

VII. Botany and Zoology, particularly with reference to geographical distribution.

VIII. Ethnography, comparative researches based on measurements, casts and photographs.

IX. Geographical aspects of India, the Himalaya, Tibet and Turkistan.

Volume I, recently received, contains a minute account of the methods of observation and calculation; with full details of the observations for the determination of geographical co-ordinates. The magnetic observations are very numerous, and are reported with great minuteness. These observations embraced the Declination of the magnetic needle; the horizontal intensity; the Dip and Vertical Intensity; and also the total intensity. The instruments employed were those generally adopted in the English magnetic surveys, and the magnetic intensity is expressed in the unit employed by General Sabine, and other English philosophers.

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* See this Journal, vol. xxvi, 378, and xxix, 236.

The following is a summary of the magnetic observations made in the course of this Survey.

In column fourth S. L. signifies that the station was but slightly elevated above the level of the sea.

Results of the magnetic observations.

Station.	Latitude.	Longitude from Greenwich.	Height Eng. feet.	Declination East.	Dip North.	Total Intensity.
Suget,	36° 10′ N.	77° 50 ′E.	12,960	4° 21′·5	50° 12′·35	
Sumgal,	36 8	78 5	13,212		50 5 ·38	10·879
Tso Ka,	35 58	76 3	15,724		49 18 ·92	
Karakorum Pass,	35 46	77 30	18,341	3 33 ·6	49 13 ·75	10·933
Chorkonda,	35 33	75 56	13,790	2 53 ·4	48 43 ·25	
Skardo,	35 20	75 44	7,250	4 5 ·1	48 20 ·52	10·943
Tashing,	35 15	74 40	9,691	4 17 ·7	48 23 ·82	10·751
Sasser Pass,	35 6	77 27	17,753	3 31 ·9	48 17 ·68	
Daver,	34 34	74 46	7,718		47 41 ·65	
Kargil,,	34 30	76 4	8,845	3 10 ·1	47 57 ·15	10·197
Dras,	34 28	75 43	9,951		46 51 ·45	10·122
Mozaferabad,	34 22	73 31	2,220	3 23 ·9	47 20 ·00	9·827
Leh,	34 8	77 14	11,527	3 22 ·6	46 52 ·64	10·113
Srinagger,	34 4	74 48	5,144	2 59 ·9	46 58 ·20	9·986
Peshaur,	34 3	71 33	1,250	2 27 ·9	46 25 ·75	10·889
Marri,	33 51	73 22	7,260	3 21 ·1	46 2 ·84	9·633
Tsomognalari,	33 39	78 38	14,010	3 21 ·8	46 34 ·05	9·972
Raulpindi,	33 36	72 59	1,674	3 5 ·5	45 55 ·71	9·904
Padum,	33 28	76 54	11,590	3 40 ·8	45 51 ·97	
Tsomoriri,	32 45	78 16	15,130	3 9 ·9	45 20 ·30	
Kardong,	32 33	77 0	10,233	3 23 ·3	44 28 ·44	10·960
Sultanpur,	31 57	77 5	3,830	3 2 ·6	43 52 ·19	
Mud,	31 55	78 1	12,421	3 43 ·5	44 17 ·83	
Dera Ismael Khan,	31 39	70 56	478	0 58 ·2	44 23 ·47	10·703
Vangtu Bridge,	31 37	77 54	4,210		43 22 ·80	
Lahor,	31 34	74 14	790	2 2 ·3	43 17 ·44	9·856
Rampur,	31 31	77 37	3,215		42 46 ·43	
Ussilla,	31 7	78 18	8.940		42 13 ·24	10·960
Simla,	31 6	77 7	7,091	2 55 ·5	42 30 ·00	9·709
Mana,	30 47	79 20	10,670	2 44 ·9	41 25 ·24	10·528
Milum,	30 34	79 54	11,640	2 40 ·3	40 31 ·91	10·489
Massuri,	30 28	77 59	7,549		41 15 ·12	10·807
Ambala,	30 21	76 48	1,026	2 26 ·2	40 48 ·40	
Nainital,	29 23	79 30	6,409	2 28 ·2	38 33 ·71	9·856
Shikarpur,	27 55	68 51	60		36 2 ·00	9·893
Aligarh,	27 53	78 3	760	1 37 ·3	36 58 ·90	
Kathmandu,	27 42	85 12	4,350	2 35 ·8	37 34 ·24	8·734
Dibrugarh,	27 32	94 57	395	0 46 ·4	38 30 ·35	9·882
Falut,	27 6	87 59	12,042	2 24 ·8	36 54 ·96	8·316
Darjiling,	27 3	88 15	7,168	2 48 ·0	36 32 ·97	8·152
Tonglo,	27 1	88 3	10,080	2 30 ·5	36 25 ·04	8·539
Narigun,	26 53	92 6	3,615	4 43 ·0	37 8 ·11	9·114
Lakhnau,	26 51	80 55	520	2 37 ·4	35 18 ·55	10·019
Sigauli,	26 46	84 44	260		35 40 ·10	
Udelguri,	26 45	91 56	352	2 36 ·3	36 27 ·65	9·624
Tezpur,	26 34	92 46	239	0 22 ·5	37 14 ·93	9·746
Kissenganj,	26 6	87 56	140	2 20 ·2	35 11 ·95	8·187
Gohatti,	26 5	91 43	134	2 0 ·1	35 19 ·15	9·541
Patna,	25 37	85 7	170	1 53 ·9	33 32 ·96	9·215
Benares,	25 18	82 59	325	1 50 ·3	32 41 ·25	9·294
Cherra Punji,	25 14	91 40	4,164	2 20 ·4	33 7 ·27	9·449
Surajganj,	24 22	89 43	S. L.		32 3 ·50	

TABLE—(continued.)

Station.	Latitude.	Longitude from Greenwich.	Height Eng. feet.	Declination East	Dip North.	Total Intensity.
Rampur Bolea,	24° 21′ N.	88° 34′ E.	54		32° 0′·77	7·904
Sager,	23 50	78 43	1,880		29 58 ·84	
Dhaka,	23 42	90 20	s. l.	2° 21′·2	31 1 ·23	
Bhuj,	23 17	69 40	283	0 12 ·0	28 25 ·00	9·109
Jablpur,	23 9	79 56	1,480	1 10 ·5	28 31 ·14	9·863
Kulna,	22 45	89 36	s. l.	2 30 ·4	29 19 ·85	
Calcutta,	22 33	88 20	s. l.	2 25 ·1	28 14 ·84	9·113
Nagri,	20 25	78 52	850		22 49 ·99	9·367
Bombay,	18 53	72 49	s. l.	0 19 ·1	19 6 ·60	8·475
Puna,	18 30	73 52	1,819		19 2 ·25	
Mahabaleshvar,	17 55	73 38	4,396		16 25 ·50	
Rajamandri,	17 10	81 46	s. l.	1 24 ·8	16 23 ·53	9·197
Kaladghi,	16 12	75 29	1,720	0 30 ·0	14 27 ·25	
Bellari,	15 8	76 53	1,580	0 21 ·0	11 59 ·68	8·834
Madras,	13 4	80 13	s. l.	0 59 ·3	7 52 ·34	8·100
Utakamand,	11 23	76 43	7,278	0 57 .0	4 27 ·32	8·862
Utatur,	11 4	78 51	280		2 50 ·08	
Galle,	6 2	80 10	s. l.	0 41 ·0	–7 40 ·90	8·076

3. *Microscopic Anatomy of the Lumbar Enlargement of the Spinal Cord;* by JOHN DEAN, M.D. (Communicated to the American Academy of Arts and Sciences by Prof. Jeffries Wyman, Nov. 14, 1860.)—This is a paper of great value, displaying extensive and patient research in a very important but little cultivated department of anatomy. This indefatigable investigator of the nervous system has confirmed the discoveries of J. L. Clarke, published in the Transactions of the Royal Society, 1859, and added other important discoveries of his own.

Our limits do not permit us to give even a synopsis of this valuable paper but we cite the following account of the author's method of preparing sections of the spinal cord for microscopic examination:

"The method of preparation usually employed was a modification of Gerlach's and Clarke's although many others were employed, according to the object in view. The following method gave the best results from which to make drawings. Thin sections from the cord, hardened in alcohol, were washed a few minutes in pure water, and then immersed in glycerine, to which Gerlach's solution of carmine (solution of carmine in water to which a few drops of strong ammonia have been added) previously filtered, had been added; in this the sections were allowed to remain from four to eight hours according to the tint desired (a light tint interfering least with the details and sharpness of outline). I have been able to obtain more delicately colored specimens and more clearly defined structure by the use of glycerine than by any other method. The sections are then washed first in pure water, afterwards with strong alcohol, in which they are allowed to remain about an hour, and are now ready for preparation with turpentine according to Clarke's method; [*i. e.*, to lay them on a slide and drop on turpentine from time to time until the alcohol is replaced by turpentine] they may be put in Canada Balsam, or, as I have found very advantageous, in thick, colorless copal varnish, which often preserves minute details better than balsam. Although Stilling and others have found much fault with Clarke's method of preparation, on account of the too great transparency it sometimes gives, I am convinced that, with practice and some slight modifications, it is the only one suited to the minute study of the cord, other methods seeming to me, after thorough trial, quite unsatisfactory as compared with Clarke's. As a hardening material, I have often employed chromic acid with considerable advantage; but when coloring matter is used, alcohol is most suitable, and is certainly much easier to succeed with."

4. *Summary of Medical Science;* edited by Walter S. Wells, M.D. 304 pages. 8vo. New York, Charles T. Evans.—This is an epitome of Medical Literature selected from European and American Medical Journals, including also original articles, to be issued semi-annually. The arrangement and design of this work will make it a valuable repertory of the current views of the medical profession, especially to those who are not supplied with a very large variety of medical periodicals.

5. *Report on the Geology and Agriculture of the State of Mississippi;* by Eugene W. Hilgard, Ph.D., State Geologist. Jackson, Miss., 1860. 8vo, pp. 391, accompanied by a Geological Map and Sections.—This Report, announced in our last, is a valuable contribution to our knowledge of the Cretaceous and Tertiary strata which cover by far the larger portion of the surface of Mississippi. We had marked for extract the remarks of Dr. Hilgard on the general relations of these two great groups of strata, as unfolded in his Report, but want of space compels us to postpone their citation now. The Geological Map of Mississippi which accompanies the Report is a valuable addition to our knowledge of the exact tendencies of the several formations in this State, made more valuable by the profiles of the Mississippi formations which are given. We hope to notice some points (both Geological and Agricultural) in this report again.

6. *First Biennial Report of the progress of the Geological Survey of Michigan*, embracing observations on the Geology, Zoology and Botany of the Lower Peninsula, made to the Governor, Dec. 31, 1860. Lansing, 1861. 8vo.—A fragment only of the complete Report (270 pages) has reached us. It is a satisfaction to see the work resumed in Michigan which Houghton so well began many years ago. In glancing at the Chapter on Peat, which occurs abundantly in Michigan, we see no allusion to the valuable researches of Prof. Johnson made for the Connecticut State Agricultural Society, the agricultural value of which is such as to entitle them to special notice. When the complete Report reaches us we shall resume our notice.

BROCHURES.

Zoological and Anatomical.—

On the Rhizopodal Fauna of the Mediterranean, compared with that of the Italian and other Tertiary deposits. By T. Rupert Jones, Esq., F.G.S., and W. K. Parker, Esq., Mem. M.S. 1860. 8vo, pp. 16.

Synopsis of American Cretaceous Brachiopoda. Proc. Acad. Nat. Sci., Jan. 1861. By W. M. Gabb.

On the genus Raphidophora, Serville; with descriptions of four species from the Caves of Kentucky, and from the Pacific Coast. Bost. Soc. Nat. Hist. By Samuel H. Scudder.

Synonymy of the Cyclades, a family of Acephalous Mollusca. Part 2. By Temple Prime. Proc. Acad. Nat. Sci. Phil., July, 1861.

Croonian Lecture.—On the Arrangement of the Muscular Fibres of the Ventricular Portion of the Heart of the Mammal. By James Pettigrew, Esq.

GEOLOGICAL.—

Documents anciens et nouveaux sur la faune primordiale et le Système Taconique en Amérique. Par M. J. BARRANDE. With two plates.

Notes on the Cretaceous and Carboniferous Rocks of Texas. By JULES MARCOU. Boston, 1861.

On the Occurrence of Flint-implements, associated with the Remains of Animals of extinct species in beds of a late geological period, in France at Amiens and Abbeville, and in England at Hoxne. By JOSEPH PRESTWICH, F.R.S., F.G.S., etc. London, 1861. 4to, pp. 40.

On the supposed identity of the Paradoxides Harlani, Green, with the Paradoxides spinosus, Boeck. By ALBERT ORDWAY.

On the Fossils brought from the Arctic Regions in 1859, by Captain Sir F. L. M'Clintock. By the Rev. SAMUEL HAUGHTON, M.A., etc.

On Cyclostigma, a new genus of fossil plants from the Old Red Sandstone of Kiltorcan, Co. Kilkenny; and on the General Law of Phyllotaxis in the Natural Orders,—Lycopodiaceæ, Equisetaceæ, Filices, etc. By the Rev. SAMUEL HAUGHTON, etc.

On the Discovery of some Fossil Remains near Bahia in South America. By S. ALLPORT, Esq.

On the Structure of the Northwestern Highlands, and the relations of the Gneiss, Red Sandstone, and Quartzite of Sutherland and Ross-shire. By JAMES NICOL, F.G.S., F.R.S.E., etc.

Address delivered at the Anniversary Meeting of the Geological Society of London, on the 15th of February, 1861; prefaced by the announcement of the award of the Wollaston Medal and proceeds of the Donation-fund for the same year. By LEONARD HORNER, Esq., F.R.S. L. & E., President of the Society. London, 1861.

MISCELLANEOUS.—

Report made to the Navy Department by the Board of U. S. Naval Engineers, to determine the relative economy of using steam with different degrees of expansion. Washington, 1861. 8vo, pp. 38.

Publications of Isaac Lea on Recent Conchology. Bibliography. January 1, 1861.

Report of the Trustees of the Museum of Comparative Zoology, 1861. Boston, 1861.

The Second Annual Report of the Trustees of the Cooper Union for the Advancement of Science and Art. January 1, 1861. New York, 1861.

New York State Agricultural College, Ovid, Seneca Co., N. Y.—Charter, Ordinances, Regulations and Course of Studies. 1861. Albany, 1861. 8vo, pp. 16.

The Theory and Art of Bread-making. A new process without the use of Ferment. By E. N. HORSFORD, Rumford Professor in Harvard University, Cambridge. 1861.

Catalogue of Telegraph Material, manufactured by Charles T. & J. N. Chester, 104 Centre Street, New York. With Appendix. New York.

Appendix to Messrs. Stevenson's Answer to Sir David Brewster's Reply regarding Dioptric Lights. By D. & T. STEVENSON, Civil Engineers. Edinburgh, 1860.

Reply to Professor Tyndall's Remarks, in his work "on the Glaciers of the Alps," relating to Rendu's "Théorie des Glaciers." By JAMES DAVID FORBES, D.C.L., LL.D., F.R.S., etc. Edinburgh, 1860.

Voyage d'André Michaux en Canada. Depuis le Lac Champlain jusqu'a la Baie d'Hudson. Par O. BRUNET. Quebec: Bureau de l'Abeille. 1861.

PERSONAL.

Correction.—In our notice of the death of Dr. Robb, p. 150, his name was incorrectly given as CHARLES—it was JAMES; and his labors in his chair commenced in 1837, not as stated in 1827.

LORING W. BAILEY, Esq., late of Harvard College—has succeeded to the chair made vacant by the death of Dr. Robb in the University of New Brunswick, at Fredrickton, N. B., British N. A.

The numerous friends of the late Prof. J. W. Bailey of West Point Military Academy, whose memory is so warmly cherished by all who knew him, will rejoice to welcome to the active walks of science a son who in kindred branches of investigation has already shown marked ability, and to know that he has already achieved so honorable a post.

P. S. *Sept. 2d.*—The telegraphic report of an earthquake at Louisville, Cincinnati and other central parts of the United States, reaches us at this moment. Will correspondents in the affected district send us their observations?

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXXIV.—*Physical Aspects of the Comet II*, 1861; by Commr. J. M. GILLISS, Supt. U. S. Naval Observatory.

THE comet was first noticed on the evening of the 30th of June at 10½ o'clock. At that time a bank of clouds obscured the nucleus, and its lengthened coma of pulsating light extending towards the zenith caused it to be mistaken for an auroral beam. This opinion of its nature was apparently confirmed by a number of luminous and vapory flocculi lying eastward of the meridian, and near the prime vertical. The following evening at about the same hour or a little later, it was again remarked, and under somewhat similar circumstances; but whilst the change of place was evident, there was no reason to suspect it a celestial body. At a later hour its character was no longer doubtful.

On the night of the 2d of July, the nucleus of the comet was near the small star 6 Ursæ Majoris, from which point the tail proceeded in two branches. The first one slightly curving towards the star α Ursæ Majoris, was only 8° or at most 10° in length. The other, eastern or main branch, was straight and narrow, and passed in nearly a vertical direction closely parallel with the stars λ and κ Draconis, near the latter of which it was the most attenuated, and not more than 1½° in width. Thence, spreading gradually, it could be followed by the eye to a point in the neighborhood of 68 and 72 Herculis, or to a distance of some 80° or 85° from the nucleus. During the night it seemed to move slowly westward, finally enveloping the two stars of Draco mentioned above.

Its light was neither uniform nor constant. For the first 8° or 10° from the nucleus the tail was remarkably brilliant, and then its light rapidly diminished in intensity until reaching its narrowest point, beyond which the gradation was almost insensible. The constancy of the light near the nucleus was interrupted at intervals by flashings or pulsations, closely resembling those of the aurora, and at these times it was remarked that the upper portion of the tail was perceptibly fainter, though this may have been simply the optical effect of contrast.

When viewed with a telescope of low power, the nucleus appeared as a small planetary disc of only a few seconds diameter. From this there emanated towards the sun a luminous sector, or fan-shaped head, terminated by a well-defined convex line. The matter proceeding from the nucleus toward the sun and forming the head, was at this line sharply repelled, and falling back on either side, formed the branches of the tail. The whole appearance of this sector, at first brilliant and well-defined, underwent various modifications as the night progressed. When first observed, it was nearly symmetrical with respect to the comet's axis. The eastern wing was perhaps a little the longer of the two. There was a dark oval spot near the middle of the fan and a little to the east of the axis: then a faint curved line or lines, concentric with the outer convex boundary, divided the mass into upper and lower strata, each with a cusp on either side. Afterwards the lines appeared broken, giving a mottled appearance to the central zone of the sector, and finally the western cusps seemed to break, and the fan-shape was transformed into a spiral whose centre was in the nucleus. Meanwhile, during the hours of observation, the dimensions of the whole mass had increased to at least double of the original size, while the outlines had become so indistinct that it was only with difficulty the general shape could be recognized.

July 3.—The nucleus was distinctly visible to the unassisted eye as early as 8 P. M., and as late as $4^h\ 10^m$ on the following morning. It might have been seen at even a later hour.

In the field of the telescope used last night, (the comet seeker), by 9 o'clock it was a brilliant stellar point just north of 35 Ursæ Majoris. In the equatorial it was enlarged to 11″·2, and was evidently elongated in a line perpendicular to the direction of its motion.

The luminous sector, or fan-shaped head preceding it had greatly changed. Four measurements placed its outer boundary line at 101″ from the nucleus. This line was parabolic rather than elliptical, but there was in its western half an irregularity as though a segment had been cut from that wing. In the early part of the evening, the posterior boundary was curved on each wing of the sector, its eastern half terminating in a sharp cusp, whilst the western one was not only irregular, as has been said, but its extremity was not as well defined, and the volume of that half was sensibly less. That boundary had notably flattened by $11^h 45^m$.

The surface of the sector was much less mottled than during the night of the 2d, and was at no time so vivid as then. The dark oval spot could not be recognized. Beyond the sector there was a dark space, and concentric with and at the distance of 396″ (measured by Mr. Ferguson) from the nucleus, a faint outer envelope was traceable. This last was a narrow band of dusky white hue, densest directly in front of the nucleus, at which point it was 12″ or 14″ broad, and from thence diminished towards the extremities of the wings both in volume and luminosity until its form was that of a slender crescent.

The coma continued much as last night, and requires comment under two heads. The lower, or main portion, extending from the nucleus to half-way between κ and λ Draconis, was markedly the brighter, and the upper part of it slightly turned to the westward. Its eastern edge, bordering close on κ, and passing to the left of λ, was best defined; its western more fringe-like and spreading from the axis. It was not, at any time, as brilliant as during last night, nor was the dark central line near the axis as marked, but was, as then, subject to fitful pulsations, at which periods the increase of light sometimes seemed wholly confined to within 12° or 14° of the nucleus, at others to flash to the utmost extremity of the coma almost instantly, and again at others, the whole volume appeared to be bent to the westward as a willow branch by the wind. The most remarkable of these pulsations occurred at $10^h 50^m$, during which the coma became extremely faint between κ and α Draconis, though it continued quite distinct above the latter star. Its greatest breadth did not exceed 4°.

α Draconis was near the axis of the longer and narrower portion. This was nowhere more than 1½° broad; was always faintest between κ and α Draconis, above which it was brighter, and could be followed in its gradually diminishing lustre to the eastward of α Ophiuchi where it was lost in the light of the milky way.

July 4.—The gazing observations of this night continued from $7^h 50^m$ until $11^h 30^m$, P. M.

At $7^h 55^m$ the cometary nature of the body was readily distinguishable, and the light of the nucleus had become visible before that of α Ursæ Majoris. When seen at a later hour it was more condensed than last night, and appeared in the comet seeker as a brilliant elongated star.

At $10^h 50^m$ the general form of the sector-shaped head preceding the nucleus was parabolic. It was less distinctly marked than during the last night. Its greatest extent from the nucleus by a mean of five measures in the direction of the axis of the comet was 140″, and in a line perpendicular thereto—the extremity of the east wing—about one-third greater, or 180″. The outline of the western half was broken as it was last night, and the surface of that wing was much mottled. 10° or 12° to the westward of the axis, and forming an angle of 45° with it, there emanated a narrow brush of light which was traceable in the field of the equatorial for 30″. The posterior edge of the sector was again curved as on the night of the 2d, that of the eastern half being an unbroken line, whilst the western one was quite irregular. Moreover, there was a fan-shaped bundle of rays to the latter, which shot from the nucleus to a distance of 18″ or 20″ in a line perpendicular to the axis of the sector.

The outer envelope so well seen on the night of the 3d could not be traced in the equatorial, and was perceptible in the field of the comet-seeker only at intervals and by indirect vision. It preserved its slender crescent outline and was concentric with the sector, but separated from it by a dark space of about 70″.

At $9^h\ 7^m$ the coma was plainly divisible into two branches of which the bifurcation was just above κ Draconis. The angle at which the western branch curved from the longer one was from 3° to 4°. The great volume of the light was of a delicate cream color, and was within 8° of the nucleus, from which it gradually diminished in brilliancy. The eastern edge was the best defined, but neither of them was sharp, and the western one was more brushy, and curved outwardly more than heretofore. Above the point of separation of the two branches, the western one spread into quite a broad fan-shape whose outer borders rapidly diminished in light as far as the line joining α Draconis and ζ Ursæ Majoris; and except during the occasional pulsations, it wholly faded from sight within five degrees upward from that line. During these periods, the curve could be traced to the cluster of stars near θ Bootis. The great bend of the curve was on or near the line joining δ Ursæ Majoris and κ Draconis. A dark space intervened between its eastern line and the eastern branch of the coma. α and ι Draconis and τ, σ, η and ε Herculis were all within that branch. This nowhere above α Draconis exceeded 1½° in breadth. It gradually diminished and again increased in brightness between the last named star and ι of the same constellation, and could be followed to the scattered light of the milky way east of α Ophiuchi. The periodical fluctuations in its brightness were not so remarkable as on either of the other evenings.

July 6.—Clouds broke away and the comet was found with the unassisted eye at $8^h\ 15^m$; it was obscured with occasional interruptions by clouds until 10^h when the sky became hopelessly overcast.

The comet was much fainter to-night. The tail was not more than 25° in length, and curved very nearly the same as on previous nights, but was broader, being nowhere less than 3° in width. The western and the main branches seemed both subdivided continually.

The luminosity surrounding the head was more extended than before, but with no perceptible outline.

The sector was much smaller and fainter, and for the greater part of the time could scarcely be discerned at all as distinct from the general mass of light. The vertex of its upper, convex side was some 5° or 10° to the right of the comet's axis, corresponding very nearly to the position of the line or brush of light seen on the 3d inst. The inner edge of the eastern cusp was better defined than that of the western,—it was concave as usual, while the western, when it could be seen at all, was quite straight. There was no trace whatever of any dark spots or lines, except that by occasional glimpses a shade could be suspected separating the outer luminosity from the sector, but even this was very doubtful.

July 7.—Although there were thin cirri extending over all that portion of the sky, the cometary nature of the body was recognizable as early as $8^h 5^m$. An hour later, the thin clouds had all disappeared.

At 10^h P. M. the light of the nucleus was brighter than that of ε Ursæ Majoris. With a power of 70, and the full aperture of the equatorial, it was, in comparison with the night of the 5th, condensed and brilliant, but not stellar.

The head, or what has hitherto been called the sector was not unlike an arrow-head in form.

The anterior boundary of the sector was more flattened than on previous evenings, and was not unlike an arrow-head greatly expanded perpendicular to the axis of length. A brush of luminous fibres extended from the nucleus across it, inclined to the west at an angle of 8° or 10°, and perceptible 20″ beyond the boundary. That wing was the smaller, and much the less regular in outline. The curve of the posterior boundary also was more flattened, but neither of them was well-defined, although the night had become perfectly clear.

The western branch of the coma could be traced from the nucleus to near θ Bootis, the eastern one to about a degree beyond ν and φ Herculis, and the latter stars were precisely in its line. At 8° from the nucleus the two were not more than 1½° to 2° broad. The great volume of light was within 10° of the nucleus, and at 20° the brilliancy of the coma did not exceed that of the milky way, west of γ Aquilæ. But its intensity was subject to great changes, when it seemed to flow from the nucleus in a stream steadily increasing for some minutes, and again as slowly fading away. At these times the dark space between the two branches of the coma became more distinct, and the optical illusion of a curve in the line of the axis more apparent. The most noteworthy of these changes was just at 11 o'clock, shortly after which hour the observations ceased.

July 8.—The nucleus was more planetary than during last evening, and was approximately of the same degree of brightness, or produced the same effect upon the eye as ζ Ursæ Majoris. It became visible soon after 8^{h} P. M.

In the equatorial, and with power 70 as used then, the sector head was much more distinct, and its anterior boundary quite circular. The posterior line was a caustic, of which the nucleus occupied one of the cusps. The eastern cusp was sharp, whilst the western continued as irregular as on previous evenings.

The marked brush or rays of light diverging from the nucleus extended across the general mottled surface of the sector at the angle observed last night.

The coma continued in two distinct branches, of which the western one was curved, and constantly traceable to within a degree of θ Bootis. During the pulsations it could be seen a degree or two beyond that point. Its western outline was markedly brushy from the nucleus to extreme visibility. The eastern branch extended to and covered υ and φ Herculis. The great volume of light was within 8° of the nucleus, and its eastern line was the sharpest. Above the point mentioned, the coma diminished in brightness uniformly, until it was no longer distinguishable beyond φ Herculis. As heretofore the dark space between the two branches was most notable during the outward emissions of light from the nucleus through them.

Upon attempting to compute an orbit for the comet, Prof. Hubbard found it impossible to represent the observed path by a parabola, and then obtained by the Gaussian method the following hyperbolic elements, based upon the places obtained by Mr. Ferguson on the 2d, 8th, and 17th of July. They give for the middle date the values $\Delta l=-0''\cdot33$, $\Delta b=-0''\cdot16$.

Time of perihelion passage,	1861, June 11.85294.	Wash. M. T.
Long. of perihelion,	249° 44′ 44″·58	M. equinox,
" " node,	278 59 49 ·72	1861·0
Inclination,	85 56 8 ·86	
Excentricity,	1·0265470	
Perihelion distance,	0·7453901	

The following is the list of observations, as far as reduced and compared with the above elements:

Wash. M. T.	α	c.—o.	δ	c.—o.
d	° ′ ″	″	° ′ ″	″
July 2·41250	130 48 6·8	+ 2·4	63 12 48·5	+ 1·3
3·36480	147 56 52·0	+17·9	66 10 2·4	− 0·6
·38102	148 14 43·1	− 9·5	66 11 49·9	− 4·7
·63908	152 46 48·8	+24·8	66 34 17·6	− 2·5
4·36786	164 40 26·8	+18·6	66 54 21·2	− 5·5
6·37421	187 49 0·3	+ 2·5	64 51 1·7	− 5·8
8·36769	200 12 23·3	− 0·5	61 50 2·0	− 0·8
10 53104	207 46 0·2	− 1·4	58 58 55·8	− 9·4
11·45260	209 59 3·0	+ 2·3	57 56 58·2	−10·2
12·42192	211 54 9·1	+ 8·9	56 58 12·1	− 7·8
14·43062	214 56 37·2	+ 7·5	55 14 25·5	− 6·1
16 37969	217 6 9·6	−13·4	53 52 5·4	+ 5·2
17·35071	217 58 33·0	− 4·8	53 16 37·0	+ 1·5
·42154	218 1 58·8	+ 4·2	53 14 13·7	− 3·8
20·40982	220 9 54·0	+16·7	51 42 28 5	− 3·8
23·37224	221 44 40·7	+19·5	50 30 17·2	− 9·1
24 35193	222 11 27·8	+28·1	50 9 26·8	− 9·8
25 36437	222 37 26·0	+32·5	49 49 15·3	−13·2
27·37080	223 24 38·3	+36·0	49 12 25·8	− 8·9

From these residuals it is evident that the orbit requires yet some correction, not sufficient, however, it is believed, to change its decided hyperbolic nature.

ART. XXXV.—*A Sketch of the Life and Scientific Services of John Evans, M.D., U. S. Geologist for Oregon and Washington Territories, and of the U. S. Chiriqui Exploring Expedition;* by CHARLES T. JACKSON, M.D., of Boston.

THE labors of pioneer geological surveyors, who visit the unbroken wilderness and scale mountains never before trod by civilized man, thread the mazes of cañons amid the Rocky Mountains, or navigate in frail bark canoes unfrequented rivers with unknown rapids and tangled with fallen trees, are of such an arduous and dangerous nature that only the boldest and most adventurous are willing to undertake or are able to accomplish them.

We look with the same interest on one who has successfully accomplished such feats in behalf of science, as we regard the war-worn veteran who has returned from a successful campaign against the enemies of his country. We know and feel what hardships he has endured, and sympathize with him in his triumphs over difficulties and dangers, while we enjoy with him the fruits of his researches, knowing how dearly they have been acquired. The enthusiastic geologist, thousands of miles away from home, throwing away much needed bread in order to preserve specimens to illustrate the mineralogy, geology, and natural history of the country he explores, kindles in us a profound respect for his self-sacrificing spirit and desire to serve, to the extent of his ability, the cause of science. Scientific explorations, in a country infested by hostile savages, require the wisdom, coolness, and intrepidity of an old border warrior to ensure success. To accomplish such feats, and return in safety, was the fortune of the subject of the present sketch, and though, unhappily for his country, he died before his valuable reports had been published, we doubt not the Government will duly make public record of his labors.

Dr. EVANS was born in Portsmouth, New Hampshire, on the 14th of February, 1812, and was son of Hon. Richard Evans, a Judge of the Supreme Court of New Hampshire. He was educated at Andover, Massachusetts, and was in his early manhood employed for a while as a mercantile clerk. In 1831 he removed to Washington, and was employed as a clerk in the general Post Office Department for eight years. He afterwards removed to St. Louis, studied medicine, and received the degree of M.D. at the Medical College of that city. In 1835 he married Miss Sarah Z., daughter of the distinguished architect, Robert Mills, formerly of Charleston, South Carolina, but for many years a resident at Washington. By this marriage he had three sons and one daughter.

In 1847 he was appointed, by Dr. David Dale Owen, to be one of his assistants in his geological survey of the western portion of the Chippewa land district, and accomplished his duties to the entire satisfaction of his employer and the public.

In March, 1851, he was appointed, by the Secretary of the Interior, "to institute geological researches on the main lines of the public land surveys about to be commenced in Oregon," and entered zealously on that laborious duty, exploring the falls of the Missouri to the Flat Head pass, in the Rocky Mountains, and through that pass from the forty-sixth to the forty-ninth degrees of north latitude, and to the Columbia River. The valleys of the Flat Head Lake and Bitter Root rivers were also explored, and he crossed the Bitter Root range of mountains, which are one hundred and twenty miles wide, and explored the country to the Spokane, Clear Water, Snake, Walla Walla, Utillah, John Days, and Falls rivers to the Columbia, and thence to Oregon City.

He collected specimens of all the rocks, minerals, fossils, and soils of those wide spread regions, and forwarded them for description and analysis to some of the most skillful mineralogists, palæontologists, botanists, and chemists of the United States, among whom we may name Dr. Owen, Professors Litton, Leidy, Shumard, and Lesquereux, all of whom were supplied with ample materials for their scientific examination and description. To the author of this paper he sent specimens of rocks for description, and of coals, ores of iron, lead and copper, and a large number of selected soils, and a piece of very remarkable meteoric iron, for analysis.

Dr. Leidy, he informs us, has furnished no less than ninety figures of mammalian fossils, and the other scientific gentlemen have worked out the problems given them to solve, so that a magnificent foundation is laid for a most interesting and valuable report, embracing the labors of some of the most able naturalists and chemists in this country.

Dr. Evans worked steadily on his report, presented it to Congress, and having had an opportunity to re-visit Oregon and Washington Territories, by consent of Congress withdrew his report for the purpose of adding to it his last researches, and thus making it more perfect. We understand that this labor was performed, and that the revised and completed report is now in the general land office at Washington, awaiting publication, which it is earnestly hoped may not long be delayed.

One of the most interesting scientific discoveries made by Dr. Evans, during his explorations in Oregon, was that of an enormous mass of meteoric iron containing an abundance of chrysolite or olivine imbedded in it. During the Indian war in that region, Dr. Evans ascended Bald Mountain, one of the Rogue River range, which is situated from thirty-five to forty miles from Port Orford, a village and port of entry on the Pacific coast, and obtained some pieces of metallic iron, which he broke off from a mass projecting from the grass-covered soil on the slope of the mountain. He was not aware of its meteoric nature until the chemical analysis was made, but the singularity of its appearance caused him to observe very closely its situation, so that when his attention was called to the subject he readily remembered the position, form, appearance, and magnitude of the mass and manifested the most lively interest in procuring it for the government collection in the Smithsonian Institution at Washington, a duty I doubt not he would have been commissioned to perform had his life been spared.

By the aid of information contained in his letters to me, perhaps some traveler in those regions may be able to find this very interesting meteorite, and I shall therefore transcribe what he says of it. In reply to my inquiry, whether he felt confident he could again find this mass of meteoric iron, he says in his letter of May 1st, 1860:

"There cannot be the least difficulty in my finding the meteorite. The western face of Bald Mountain, where it is situated, is, as its name indicates, bare of timber, a grassy slope, without projecting rocks in the immediate vicinity of the meteorite. This mountain is a prominent landmark, seen for a long distance on the ocean, as it is higher than any of the surrounding mountains. It would doubtless be best and most economical to make a preliminary visit to the locality, accompanied only by the two voyageurs alluded to in my last letter." (Two of the Canadian Frenchmen in employ of the Hudson Bay Company.)

"Arrangements might then be made with the Indians for its purchase, and the best plan selected for its removal. It would be expedient to procure the men and animals necessary in the Umpqua valley, east of the Coast Range of mountains, as Port Orford at present is quite a small settlement, although a 'port of entry.' The meteorite might be shipped in the California steamer to San Francisco, and from that port in a sail vessel round the Horn to Boston."

Dr. Evans estimates the appropriation required at from two to three thousand dollars.

At the General Land Office and Indian Bureau in Washington, Dr. Evans ascertained that the right to this meteorite "vests in the Indians," the land not yet having been ceded to the United States, and any agreement made with them would be binding, and the United States government could lay no claim to it. Dr. Evans then adds:

"There would not be the slightest difficulty in making an arrangement with the Indians, for I am personally acquainted with their chiefs. The principal chief, 'Old John,' spent several days in my camp of two men, during the height of the war, when it would have been dangerous for less than three or four companies of U. S. soldiers to have passed the same mountains. It would require only ten or a dozen blankets, tobacco, &c., as presents."

"As to the cost of transportation of the meteorite to Port Orford, it is difficult to make an accurate estimate. It is situated in a mountainous region, thirty to thirty-five miles from the coast, and the only access to it is by mountain trails. It might be removed in pieces of from one hundred to one hundred and fifty pounds in weight on pack mules; and accurate measurements made of the whole mass without great expense, say from $1,200 to $1,500. But to remove it entire would either be impracticable or involve great expense, unless indeed a river which passes the base of the mountain (Sixe's River), and empties into the Pacific, should prove navigable for a raft of sufficient size for its transportation. There is water enough, but it is no doubt much obstructed by fallen timber, and may have rapids, which it would be difficult to pass over with such a heavy load. In either mode of transportation my first duty would be to explore this river."

In another letter Dr. Evans says: "As to the dimensions of the meteorite I cannot speak with certainty, as no measurements were made at the time. But my recollection is, that four or five feet projected from the surface of the mountain, that it was about the same number of feet in width, and perhaps three or four feet in thickness; but it is no doubt deeply buried in the earth, as the country is very mountainous, generally heavily timbered, and subject to washings from rains and melting of snow in the spring, so that in a few years these causes might cover up a large portion of it. The mass exposed was quite irregular in shape. * * * *

In another letter Dr. Evans states concerning the meteoric iron:

"The locality is about forty miles from Port Orford, in the mountains which rise almost directly from the coast, only accessible by pack mules. But each mule might carry three hundred pounds weight, and if required make several trips, to secure the whole mass. It would, however, be necessary to take along suitable tools, to separate the mass, which might if desirable be adjusted together afterwards. But I should suppose that each institution, which might furnish the funds, would desire a portion of the mass."

The latter remark refers to his proposal to raise the funds from several scientific Societies, in case that Congress should fail to appropriate money for the purpose of procuring the meteorite. The legal proprietorship in it was also inquired about expressly with this view in case Congress should fail to grant funds for procuring this very interesting object of science.*

Every possible exertion was making in Congress, and with the departments at Washington, to induce the government to take measures for procuring this very valuable meteorite, and to cause it to be placed in the museum of the Smithsonian Institution, where it could readily be examined by scientific men, but Dr. Evans death, and the present unhappy state of the country, seem to prevent the realization, for the present, of this enterprize.

Dr. Evans in one of his letters to me writes also:—"Did I ever mention to you the discovery made by me (in 1853) of platinum on the Pacific coast?—A portion of the black sand, collected from the bed of a small creek, yielded 47 per cent, as analyzed and separated by Dr. Litton."

Dr. Evans also states that "on the Western slope of Bitter Root Mts., bordering on the Rooskooski River, a large tributary of the Columbia, I discovered a mountain of iron ore, a surface specimen of which yielded, on analysis, 67 per cent of metallic iron. It is fully equal in richness and, greater in altitude, than the celebrated Iron Mountain of Missouri."

Without anticipating more of the matters, in Dr. Evans' Report, which we hope may soon be published by Government, I may say that, from the specimens examined by me, it is evident that there are vast regions of volcanic origin in Oregon, that trachytes, basalt, lavas, native sulphur, and other materials of igneous origin, were among those sent to me for description or analysis, and there were also specimens of pure rock salt, which the note accompanying the specimen stated was "in great abundance," as also was the native sulphur.

Argentiferous lead ores, and specimens of yellow copper pyrites, were also discovered, besides numerous ores of iron and many tertiary dry coals, before mentioned.

Dr. Evans adds: "Is it not worthy of note in the history of nations, and is it not a great honor to our national Legislature, and the Officers of the

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* If the Bald Mountain meteoric iron is like in tenacity and hardness to that of most known masses of similar origin, the dissection of it would prove a task of far more difficulty than seems to have been supposed by Dr. Evans: so great indeed that in our judgment it would be impracticable, to the extent suggested. If its dimensions—which appear to be quite conjectural—are not something excessive (like those of the great Columbia and Brazilian irons) the cost of transporting it entire the moderate distance of thirty or forty miles would certainly be less than that of its dissection. It would not be difficult, with the aid of a good mechanic, to contrive a vehicle which would both raise it from its bed and effect its transport.—Eds.

General Government, that they should have instituted and completed, a Geological reconnoissance of the vast regions between the Western States and the Pacific Ocean, and from the British possessions to the boundary of California? This survey, exhibiting as it does by careful analyses the constituents of all the prevailing soils, coals, and minerals: and that too whilst the country for the most part is still inhabited by savages!

"Extensive beds of semi-anthracite (or perhaps more properly semi-bituminous) coals, ores of iron, lead, copper platinum, gold, sulphur, rock salt, &c. &c., have been discovered, in various localities, which will prove of great commercial advantage in view of the important trade between China and the islands of the Pacific and western coast."

In the prosecution of his surveys, Dr. Evans was assisted by Dr. B. F. Shumard, of St. Louis, who has kindly placed in my hands a large amount of information, concerning the discoveries made on his surveys, and also an account of Dr. Evans' discoveries in the *Mauvaises Terres*, of a vast number of bones representing the ancient and extinct fauna of Nebraska, a region explored by Dr. Evans, while in the employ of Dr. D. D. Owen, researches from which Dr. Leidy derived those interesting materials, for his very valuable memoir. The attention of scientific men was first called to the *Mauvaises Terres*, as a charnel house of fossil remains, by Dr. Hiram A. Prout, (see this Journal, [2], vol. ii, page 248).

To Dr. Evans is also due the discovery of the Pass of the Rocky Mountains, named after Cadotte, who is not its original discoverer, as we are assured by Dr. Shumard, as well as by Dr. Evans himself.

We omit the very interesting descriptions, which, both Dr. Evans and Dr. Shumard have given of their wilderness adventures, trials and perils; but before closing this paper, I would say that Dr. Evans, in his journey across the Rocky Mountains, met with the usual misfortunes which attended all early travellers in that desolate region:—that he was reduced to the necessity of killing and eating his pet dog after having, in his own words, "thrown away bread to preserve stones:"—that his mules gave way under labor and privations, amid these fastnesses, and were left to die, while our geologist, braving all dangers and fatigues, forced his way over the mountains and reached his destination in safety.

Dr. Evans, fortunately for himself, acquired a high renown among the Indians, for his supposed power of curing the cholera, and his Indian name means, the medicine man who kills the cholera, he having been fortunate in his treatment of the cases, which came under his care. On one occasion, a delegation of chiefs waited upon him, and besought him to "make medicine," (perform some mysterious rite), so that the Great Spirit would send them plenty of Buffaloes, protect their corn from frost, and give them a moderate winter." They fully believed in his great influence with the celestial powers, and to this superstition, undoubtedly, he owed his life in his adventurous travels, amid the war paths of hostile Indians. He was informed that all the chiefs of the tribes west of the Rocky Mountains knew him; for they have a way of telegraphing intelligence to great distances, without the aid of electricity. Dr. Evans's sextant, on one occasion, certainly saved his life from predatory Indians, who came to kill him and his party, but were awed by his bringing down the sun at his will, and they went away and reported that they dared not kill him; for some great misfortune would be sure to fall upon the tribe if they injured so great a medicine man. Large parties of miners have been killed when endeavoring to follow the paths which Dr. Evans passed in safety, and a party of a dozen of them were killed soon after Dr. Evans left Rogue River Mountains when they endeavored to visit the spots he had explored.

Dr. Evans went to Central America last year as geologist to the Chiriqui Exploring Expedition. He there discovered, in the lagoon districts of the Gulf of Chiriqui, an abundance of excellent bituminous coals of the Eocene Tertiary age, the aggregate thickness of the beds being 73½ feet, while their mean thickness was not far from four or five feet. Six of the beds, he states, are so contiguous one over the other as to be mined together in a thickness of clear coal of thirty feet.

Such a deposit, of good coals, was of vast importance to the enterprise then in contemplation, of opening a new route across the Isthmus to the Pacific Ocean—a project which we trust will be revived, when the unhappy war in which the country is now engaged shall be brought to an end.

These coals, analyzed by the writer, were found to be of excellent quality, and suitable for all the uses to which good bituminous coals are applied. These Tertiary coals undoubtedly contain succinic acid, for they give out the fragrant odor of burning amber when heated. They differ from the lignites of northern Tertiary beds entirely, and seem to point to a condition of things such as must have existed during the older and regular Coal formation.

The gorgeous scenery of the tropical forests, and luxuriance of vegetation in Central America, seem to have strongly impressed Dr. Evans, and he expressed in his letters to the General Land Office an intention of removing with his family to Chiriqui. A dangerous delusion! for the destructive effects of that seductive climate have proved fatal to nearly all the northern explorers who have labored there, and we have no doubt that Dr. Evans' constitution was enfeebled by his exposure in that region during the summer months and the rainy season, so as to render him incapable of resisting the very mild attack of pneumonia, which ended his career by death, in Washington, April 13th, 1861.

He died in the midst of his labors, and before they had been made public; but his work had reached a state to ensure it from being lost to the world. A simple resolution of Congress is now all that is needed to bring forth from the government press his report containing, as we believe, matter of value in this country alike to science and the general public.

Boston, September 1, 1861.

ART. XXXVI.—*On the great Auroral Exhibition of Aug. 28th to Sept. 4th*, 1859, *and on Auroras generally.*—8TH ARTICLE; by ELIAS LOOMIS, Professor of Natural Philosophy and Astronomy in Yale College.

SINCE the publication of my seventh article on the great auroral exhibition of Aug. 28th to Sept. 4th, 1859, I have received from Prof. Hansteen a copy of the observations made at Christiania, Norway, corresponding to those made at Hobarton, as given in this Journal, vol. xxxii, p. 81. These observations are published in the Memoires de l'Academie de Belgique, tome xx, pp. 103–116, and Bulletins de l'Academie Royale de Belgique, tome xxi, pp. 284–298.

Observations of the Aurora at Christiania, Norway, lat. 59° 54′, long. 10° 43′ E. Magnetic dip in 1859, 71° 18′.

Day.		Hour.	Notices of Auroras.
1841.	March 15,	10^h	Aurora.
	March 22,		Rain.
	May 17,		Rain.
	July 20,		No aurora visible.
1842.	Feb. 18,	11–14	Aurora faint.
	April 11,	9–15	Slight aurora. Faint arch at 15^h.
	April 12,	9–14	Rays and flames extending to the zenith.
	April 13,	9–13	Rays and flames.
	April 15,	11–15	Flaming aurora.
	July 2,	10 and 12	The bifilar magnetometer was quite out of scale.
1844.	April 17,	9	Faint aurora, extending nearly to the zenith.
1846.	Sept. 22,	7–15	Vehement flames over three-fourths of the heavens. Reddish. Corona imperfect.
1847.	April 21,	11–14	Flaming and radiating aurora.
	Sept. 24,	7–10	Corona formed. Rays of a dark red color.
	Sept. 26,	10	Magnificent arc, radiating.
	Oct. 22,	10	Rain.
	Oct. 23,	6	High aurora, radiating behind clouds.
	Oct. 24,	5½–12	One of the most brilliant auroras we have observed. Corona formed. Vivid colors, red and yellow.
	Dec. 19,	10	Strong aurora, yellow rays, red masses without motion.
1848.	March 24,		Rain.
	April 5,	10	Faint aurora.
	April 7,	10	Arc radiating.
	Oct. 18,	7¼–10	Vigorous radiation over the whole vault. Red color very intense.
	Nov. 19,	10	Aurora.
	Dec. 22,	10	Faint aurora.

We thus see that in twenty-one cases out of thirty-four, an aurora was recorded at Christiania within twenty-four hours of an aurora at Hobarton; and considering the number of auroras which must be rendered invisible by clouds and by day-light, we may safely conclude that almost every auroral exhibition at Hobarton is accompanied by a nearly simultaneous exhibition in Norway.

In successive numbers of this Journal, commencing with November, 1859, we have given a full report of observations upon the great auroral exhibition of August and September, 1859. This display was probably unsurpassed by any similar phenomenon on record, not only for its magnificence, but also for its geographical extent; and fortunately we have a greater amount of information respecting it, than was ever collected respecting any former aurora. These observations afford the materials for settling many questions which have hitherto been regarded as open to debate.

The aurora of Aug. 28th was witnessed throughout Oregon and California, longitude 124° W.; in Utah and New Mexico, longitude 111° W.; from Kansas, long. 95° W., to Maine, long. 70° W.; at Halifax, long. 63° W.; on the Atlantic Ocean in long. 45° W., 27° W., and 10° W.; and in Europe from longitude 2° W. to 18° E. Also in Asia from long 60° E. to 119° E., the disturbance of the magnetic instruments was very remarkable, although being generally cloudy, no mention was made of the auroral light. It hence appears highly probable that this auroral display extended to every meridian of the northern hemisphere. The aurora of Sept. 2d was observed at the same stations as that of Aug. 28th, besides which we have learned that this aurora was witnessed at the Sandwich Islands in long. 157° W., and from Eastern to Western Asia the disturbance of the magnetic instruments was well nigh unprecedented for its violence, so that we cannot doubt that this display extended to every meridian of the northern hemisphere.

The auroral display in the southern hemisphere was cotemporaneous with that in the northern, and was perhaps equally remarkable. Both of these auroras were observed in South America and in Australia, in latitudes where such exhibitions are extremely rare.

The southern limit of these auroral displays was not the same upon all meridians. In North America, the aurora of Aug. 28th appeared in the zenith as far south as lat. 36° 40′; and it attracted general attention as far south as lat. 18°. In Central Europe, this aurora extended to the zenith of places as far south as about lat. 45°. It was brilliant at Rome in lat. 42°, but was not noticed at Athens in lat. 38°; neither was it seen in Western Asia in lat. 40°.

In North America, the aurora of Sept. 2d appeared in the zenith at places as far south as lat. 22½°, and attracted general attention in lat. 12°; and if the sky had been clear, some traces of the aurora might probably have been detected even at the equator. In Europe this aurora was noticed at Athens, in lat. 38°. Both of these auroras conformed to the general law of auroral distribution, as developed in this Jour., vol. xxx, pp. 89–94, the region of greatest auroral action being in America about 15° further south than in eastern Europe.

We have been able to collect sufficient materials for determining with tolerable precision the height of these auroral displays above the earth's surface. At the most southern stations, the aurora rose only a few degrees above the northern horizon; at more northern stations, the aurora rose higher in the heavens; at certain stations it just attained the zenith; at stations further north the aurora covered the entire northern heavens, as well as a portion of the southern; and at places further north the entire visible heavens, from the northern to the southern horizon, were overspread with the auroral light. The following table presents a summary of a few of the most definite observations on the aurora of Aug. 28th, 1859, at about 8ʰ 42ᵐ P. M., New Haven time.

TABLE I.

Locality.	Latitude.	Extent of auroral display.	Authority.
North side of Jamaica,	18° 20′	Like the light of a fire.	A. J., v. 29, p. 265.
Inagua, Bahamas,	21 18	Remarkably brilliant.	" v. 29, p. 264.
Havanna, Cuba,	23 9	Rose 23° above the north horizon.	" v. 28, p. 404.
Key West, Florida,	24 33	Rose about 30° " "	" v. 30, p. 349.
Savannah, Georgia,	32 5	Rose some 45° " "	" v. 29, p. 262.

The following table presents a summary of observations of the same aurora, made at the same hour, at places where the auroral light covered the entire northern heavens as well as a portion of the southern.

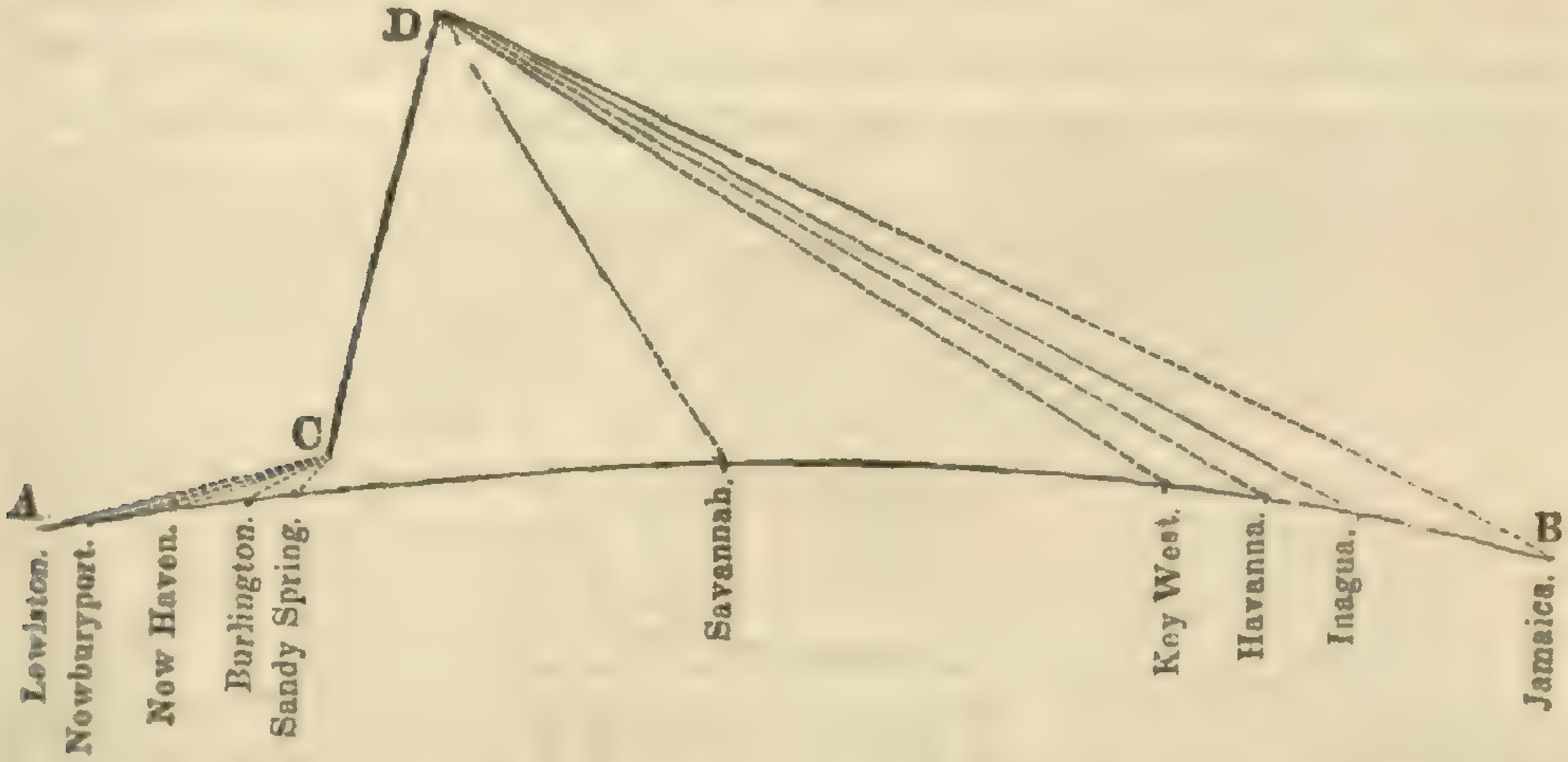
TABLE II.

Locality.	Latitude.	Extent of auroral display.	Authority.
Sandy Spring, Md.,	39° 9′	Extended to 51° from south horizon.	A. J., v. 29, p. 259.
Gettysburgh, Pa.,	39 49	" 30 " "	" v. 30, p. 345.
Philadelphia, Pa.,	39 57	" 22½ " "	" v. 29, p. 259.
Burlington, N. J.,	40 5	" 20 " "	" v. 29, p. 258.
New Haven, Conn.,	41 18	" 10½ " "	" v. 28, p. 391.
West Point, N. Y.,	41 23	" 12 " "	" v. 28, p. 394.
Newburyport, Mass.,	42 48	" 6 " "	" v. 29, p. 254.
Lewiston, Maine,	44 5	" 5 " "	" v. 28, p. 386.

If we combine the preceding observations in Table II. we shall find that *the lower limit* of the auroral light was elevated forty-six miles above the earth's surface, and that its southern margin was vertical over the parallel of 38° 50′ N. latitude in Virginia.

Now it is considered as established that the auroral streamers are luminous beams sensibly parallel to the direction of the dipping needle. But the dip of the needle in lat. 38° 50′ in Virginia is 71° 20′; and if we draw a line CD, figure 1, making an angle of 71° 20′ with the curve line AB which represents a portion of the earth's surface, we may assume that the line CD represents the southern boundary of the auroral illumination. If then we assume that the observations of Table I. were made upon the point D, we shall find that *the upper limit* of the auroral light was elevated 534 miles above the earth's surface, and that its southern margin was vertical over the parallel of 36° 40′ north latitude in Virginia.

Fig. 1.



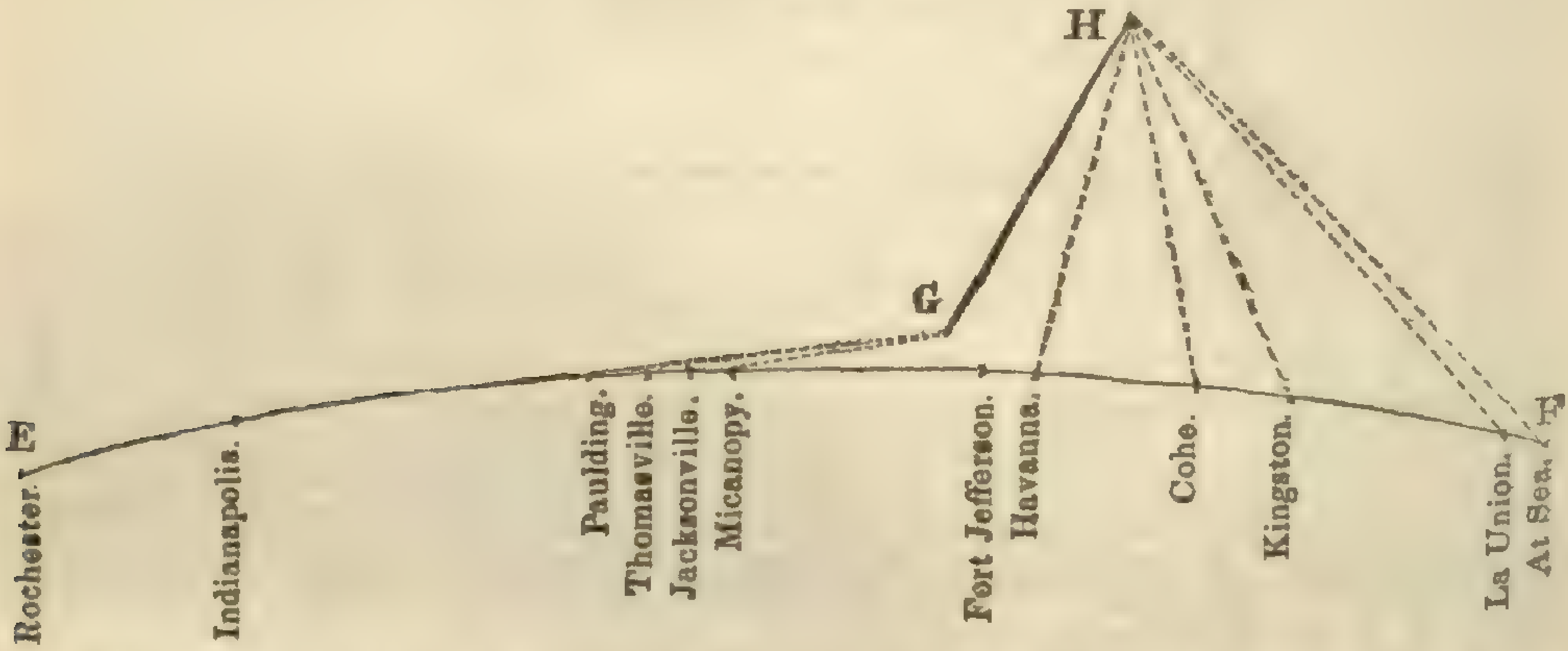
The following table presents a summary of the most definite observations of the aurora of Sept. 2, 1859, made generally about 2 A. M. Havanna time.

TABLE III.

Locality.	Latitude.	Longitude.	Hour.	Extent of auroral display.	Authority.
	° ′	° ′			
At Sea,	12 23	88 28	midnight	Sky lurid—wavy appearance.	A. J., v. 30, p. 361.
LaUnion,SanSalvador,	13 18	87 45	10–3 A.M	About 30° above the North horizon.	v. 29, p. 265.
Salvador,	13 44	88 55		Same as at La Union.	v. 29, p. 266.
Kingston, Jamaica,	17 58	76 50	1–5 A.M.	Appeared like a colossal fire.	v. 29, p. 265.
Cohe, Cuba,	20 0	76 10		Extended upwards about 72°.	v. 29, p. 265.
Havanna, Cuba,	23 9	82 22	2 A. M.	More than 100° in height.	v. 28, p. 405.
Fort Jefferson, Fla.	24 37	82 52	2 A. M.	Extended beyond the zenith.	v. 30, p. 360.
Micanopy, Fla.	29 30	82 18	2·30 A.M.	Corona very distinct.	v. 30, p. 360.
Jacksonville, Fla.	30 15	82 0	3 A. M.	Extreme south in a red glow.	v. 30, p. 359.
Thomasville, Ga.	30 50	84 0	2 A. M.	Corona formed.	v. 30, p. 358.
Paulding, Miss.	32 20	89 20	2·10 A.M.	Whole visible heavens overspread.	v. 30, p. 357.
Indianapolis, Ind.	39 55	86 5		Down to south horizon.	v. 28, p. 398.
Rochester, N. Y.	43 8	77 51	2 A. M.	Down to south horizon.	v. 29, p. 253.

If we combine the last seven observations of the preceding Table, we shall find that *the lower limit* of the auroral light was elevated fifty miles above the earth's surface, and that its southern margin was vertical over the parallel of 25° 15′ north latitude in Florida. Now the dip of the magnetic needle in Florida in latitude 25° 15′ is 55° 40′; and if we draw GH, figure 2, making an angle of 55° 40′ with the curve line EF, which represents a portion of the earth's surface, and assume that the line GH represents the southern boundary of the auroral illumination, and that the first five observations of Table III. were made upon the point H, we shall find that *the upper limit* of the auroral light was elevated 495 miles above the earth's surface, and that its southern margin was vertical over the parallel of 22° 30′ N. latitude in Cuba.

Fig. 2.



We have thus discovered the geographical position of this auroral light. The aurora of Sept. 2d formed a belt of light encircling the northern hemisphere, extending southward in North America to lat. 22½°, and reaching to an unknown distance on the north; and it pervaded the entire interval between the elevations of 50 and 500 miles above the earth's surface. This illumination consisted chiefly of luminous beams or columns, everywhere parallel to the direction of a magnetic needle when freely suspended; that is, in the United States, these beams were nearly vertical, their upper extremities being inclined southward at angles varying from 15° to 30°. These beams were therefore about 500 miles in length; and their diameters varied from five to ten and twenty miles, and perhaps sometimes they were still greater.

These beams were simply illumined spaces, and the illumination was produced by a flow of electricity. That this illumination was produced by electricity is proved by the observations of the magnetic telegraph. During these auroral displays, there were developed on the telegraph wires electric currents of sufficient power to serve as a substitute for the ordinary voltaic battery. That the agent thus excited upon the telegraph wires was indeed electricity, is abundantly proved. Electricity produces various effects by which it may be distinguished from all other agents.

1. In passing from one conductor to another, electricity exhibits a spark of light. During the auroras of Aug. 28th and Sept. 2d, brilliant sparks were drawn from the telegraph wires, even when no battery was attached. At Springfield, Mass., a flash was seen about half the size of an ordinary jet of gas. (This Jour., xxix, 95). At Washington, D. C., a spark of fire jumped from the forehead of a telegraph operator when his forehead touched a ground wire. (This Jour., xxix, 97.) At Pittsburgh, Pa., streams of fire were seen when the telegraph circuit was broken. (Ib., xxix, 97.) At Boston, Mass., a flame of fire followed the pen of Bain's chemical telegraph. (Ib., xxix, 93.) On the telegraph lines of Norway, sparks and uninterrupted discharges were observed. (Ib., xxix, 388.) Bright sparks were noticed on the conductors of the telegraph lines to Bordeaux in France. (Ib., xxix, 392.)

2. In passing through poor conductors, electricity develops heat. During the auroras of Aug. 28th and Sept. 2d, paper and even wood were set on fire by the auroral influence alone. At Pittsburgh, Pa., the magnetic helices became so hot that the hand could not be kept on them. (Ib., xxix, 97.) At Springfield, Mass., the heat was sufficient to cause the smell of scorched wood and paint to be plainly perceptible. (Ib., xxix, 96.) At Boston, Mass., a flame of fire burned through a dozen thicknesses of paper. The paper was set on fire and produced considerable smoke. (Ib., xxix, 93.) On the telegraph lines of Norway, pieces of paper were set on fire by the sparks of the discharges from the wires; and the current was at times so strong that it was necessary to connect the lines with the earth in order to save the apparatus from destruction. (Ib., xxix, 388.)

3. When passed through the animal system, electricity communicates a shock which is quite peculiar and characteristic. During the auroras of Aug. 28th and Sept. 2d, some of the telegraph operators received severe shocks when they touched the telegraph wires. At Philadelphia, the current gave a severe shock. (Ib., xxix, 96.) At Washington, D. C., the telegraph operator received a severe shock which stunned him for an instant. (Ib., xxix, 97.)

4. A current of electricity develops magnetism in ferruginous bodies. The aurora of Sept. 2d developed magnetism so abundantly and so steadily that on several lines it was used as a substitute for a voltaic battery in the ordinary business of telegraphing. (Ib., xxix, 94, 96 and 97.) The intensity of this effect was estimated to have been at times equal to that of 200 cups of

Grove's battery. (Ib., xxix, 93.) In Switzerland, the currents were at least three fold the ordinary current employed in telegraphing. (Ib., xxix, 396.)

5. A current of electricity deflects a magnetic needle from its normal position. In England, the usual telegraph signal is made by a magnetic needle surrounded by a coil of copper wire, so that the needle is deflected by an electric current flowing through the wire. Similar deflections were caused by the auroras of Aug. 29th and Sept. 2d, and these deflections were frequently greater than those produced by the telegraph batteries. (Ib., xxxii, 74.)

6. A current of electricity produces chemical decompositions. During the display of Sept. 2d, the auroral influence produced the same marks upon chemical paper as are produced by an ordinary voltaic battery; that is, the auroral influence decomposed a chemical compound, the cyanid of potassium. (Ib., xxix, 95.) The same effect was produced by the aurora of Feb. 19, 1852. (Ib., xxix, 93.)

It is thus abundantly proved that the fluid developed by the Aurora on the telegraph wires was indeed electricity. This electricity may be supposed to have been derived from the Aurora either by transfer or by induction. If we adopt the former supposition, then the auroral light is certainly electric light. If we adopt the latter supposition, then we must enquire what known agent is capable of inducing electricity in a distant conductor. We know of but two such agents, Magnetism and Electricity. But the auroral fluid was luminous, while magnetism is not luminous. We seem then compelled to admit that the auroral light is electric light.

Admitting then that the Aurora is but an effect of electric currents, it is important to determine in what direction these currents flow, and what laws they observe. Do these currents move in a vertical, or horizontal direction, or in some intermediate direction? Is there any uniformity in the direction of these currents? Our most important means of information upon this subject are derived from the observations upon telegraph lines.

The observations published in this Journal, vol. xxix, pp. 92–97, show that on a large number of telegraph lines in the United States, the electric currents moved alternately to and fro. Such was the case upon the line from Boston to Portland running N. 24° E.; from Boston to Manchester running N. 25° W.; from Boston to Cambridge almost due West; from Boston to Springfield S. 79° W; from South Braintree to Fall River running S. 12° W; from Boston to New Bedford running S. 7° E.; from Springfield to Albany running N. 58° W.; from New York to Philadelphia running S. 49° W.; from Philadelphia to Pittsburgh running N. 82° W.; and from Washington to Richmond running S. 15° W.

Now whatever may be the direction of the current on the surface of the earth, it is evident that if this current travels on a telegraph wire, it must appear to move in the direction of the wire; and a current moving across the earth's surface in any fixed direction might be forced to travel over telegraph lines making various angles with this direction; but its efficiency would vary according to the inclination of the conducting wire to the direction of the current. The following table shows the effect of a current assumed to move from N. 45° E., to S. 45° W. Column first contains a list of the telegraph lines; column second shows their directions; column third shows the angle which the assumed current makes with each telegraphic line; and column fourth shows the fraction of the entire current which would be efficient upon such a line.

Telegraph lines.	Direction.	Inclination of current.	Efficient current.
Boston to Cambridge,	West.	45°	0·71
Philadelphia to Pittsburgh,	N. 82° W.	53	·60
Springfield to Albany,	N. 58 W.	77	·22
Boston to Manchester,	N. 25 W.	70	·34
New Bedford to Boston,	N. 7 W.	52	·62
Fall River to Braintree,	N. 12 E.	33	·84
Richmond to Washington,	N. 15 E.	30	·87
Boston to Portland,	N. 24 E.	21	·93
Philadelphia to New York,	N. 49 E.	4	·99
Springfield to Boston,	N. 79 E.	34	·83

We thus see that on one-half of these telegraph lines a current assumed to proceed from N. 45° E. would exert nearly its entire force; and on only two of them would so small a part as one half of its entire force be exerted. From Boston to Manchester, only one third of the entire current would be efficient, and this would perhaps be sufficient to explain the effects mentioned in vol. xxx, p. 95. From Springfield to Albany only one fourth of the entire current would be efficient. If this should be thought inadequate to explain the facts mentioned in vol. xxx, p. 95, it may be necessary for us to admit, that the direction of the electric current was subject to occasional fluctuations. If the force of the electric current upon each of the telegraph lines had been actually measured by a galvanometer, we should probably be able to determine whether the direction of the current was invariable, and what was its prevalent direction. At present we can only infer that all the facts reported are consistent with the supposition of electric currents moving to and fro on the earth's surface, whose average direction was from about N. 45° E. to S. 45° W.

The observations published in this Journal, vol. xxxii, pp. 74–96, give us more definite information respecting the strength of the currents as well as their direction. Between Ashford and Margate there were recorded 36 north currents and 31 south currents; from Ashford to Ramsgate 24 north currents and 19 south currents; and from Margate to Ramsgate nine north currents and five south currents; that is, currents from north to south were somewhat more frequent than currents from south to north. Between Ashford and Margate the northerly currents were on an average one degree stronger than the southerly; between Ashford and Ramsgate the southerly currents were on an average four degrees stronger than the northerly; while between Margate and Ramsgate the northerly currents were on an average six degrees stronger than the southerly. Mr. Charles V. Walker from a discussion of these and other similar observations has arrived at the conclusion that in the S.E. part of England, *there is a stream of electricity of indefinite width drifting across the country, moving to and fro along a line directed from N. 42° E. to S. 42° W.*

Now it is well known that an electric current has the power of deflecting a neighboring magnetic needle; the needle always tending to take up a position at right angles to the direction of the current; and if the direction of the current be reversed, the north pole of the magnetic needle will be deflected in a direction contrary to what it was in the first case. Mr. C. V. Walker has compared the magnetic observations made at Greenwich and Kew, and has discovered that the deflections of the magnets there observed were such as should be produced by the electric currents observed on the telegraph wires, (Proc. Roy. Soc., Feb. 14, 1861). We may then employ observations of the magnetic needle as indicating the direction and force of the electric currents near the earth's surface.

In the year 1835, there was formed in Germany a Magnetic Union, which included Philosophers from every part of Germany, and which in a few years spread over nearly every part of Europe. The object of this Association was to make simultaneous observations of the magnetic needle. The observations were all made in Göttingen mean time, at intervals of five minutes for a period of 24 hours on certain days of the year previously agreed upon. These observations were annually published in a volume entitled 'Resultate aus den Beobachtungen des magnetischen Vereins,' and afford the best materials we have for comparing the effect of electric currents over large portions of the earth's surface. These observations have been projected in curves which exhibit to the eye at a glance the movements of the magnetic needle at each station. On comparing these curves, we find a remarkable similarity at places widely separated from each other. From Göttingen to Munich (distant in a straight line more than 250 miles) the curves are ordinarily almost parallel to each other; and the changes take place sensibly at the same instant of absolute time, with this modification, that the extent of the deflections is generally somewhat greater at the more northerly stations. I have made a careful comparison of these observations for the purpose of determining whether these movements of the magnetic needles were strictly simultaneous. The following catalogue exhibits a list of those cases which afford the most satisfactory data for comparison, viz., when there was a well marked maximum or minimum value of the magnetic declination, and when this maximum or minimum value was of short duration. In the following list, all the dates are expressed in the mean time of Göttingen.

Observed deflections of the horizontal magnetic needle.

d h m
1. 1836. Aug. 17. 7 50, Maximum at Upsala, Berlin, Göttingen, Leipsic and Munich.
7 55, Maximum at Hague.
2. 1836. Aug. 17. 9 50, Max. at Upsala, Berlin, Hague, Göttingen, Leipsic and Munich.
3. 1836. Aug. 17. 10 10, Minimum at Upsala, Leipsic and Munich.
10 15, Min. at Berlin, Hague and Göttingen.
4. 1836. Aug. 17. 10 30, Max. at Berlin, Leipsic and Munich.
10 35, Max. at Upsala, Hague and Göttingen.
5. 1836. Sep. 24. 8 55, Max. at Upsala, Berlin, Hague, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.
6. 1836. Sep. 24. 11 0, Min. at Hague.
11 5, Min. at Upsala, Berlin, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.
7. 1836. Sep. 24. 18 40, Max. at Upsala, Berlin, Hague, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.
8. 1836. Sep. 24. 18 50, Min. at Upsala, Berlin, Hague, Göttingen, Breslau, Leipsic, Marburg and Munich.
18 55, Min. at Milan.
9. 1836. Sep. 24. 19 0, Max. at Upsala, Berlin, Hague, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.
10. 1836. Sep. 24. 21 10, Max. at Upsala.
21 15, Max. at Berlin, Hague, Göttingen, Breslau, Leipsic, Marburg and Munich.
21 20, Max. at Milan.
11. 1837. Jan. 28. 8 55, Max. at Upsala, Altona, Berlin, Göttingen, Breslau, Freiberg, Augsburg, Munich and Milan.
12. 1837. Jan. 28. 9 30, Min. at Upsala, Altona, Berlin, Göttingen, Breslau, Leipsic, Freiberg, Marburg, Augsburg, Munich and Milan.
13. 1837. Jan. 28. 12 30, Max. at Upsala, Altona, Berlin, Breda, Göttingen, Leipsic, Breslau, Freiberg, Marburg, Augsburg. Munich and Milan.
14. 1837. Jan. 28. 21 30, Max. at Upsala, Altona, Berlin, Breda, Göttingen, Leipsic, Breslau, Freiberg, Marburg, Augsburg, Munich and Milan.
15. 1837. May 28. 9 45, Max. at Copenhagen.
9 50, Max. at Upsala, Berlin, Breda, Göttingen, Breslau and Marburg.
9 55, Max. at Munich and Milan.
10 0, Max. at Leipsic.
16. 1837. July 29. 6 20, Max. at Petersburgh.
6 35, Max. at Upsala and Copenhagen.
6 40, Max. at Berlin, Breda, Göttingen, Breslau, Leipsic, Freiberg, Marburg, Munich and Milan.
17. 1837. July 29. 7 0, Min. at Petersburgh.
7 15, Min. at Upsala, Copenhagen and Breslau.

d h m

1837. July 29. 7 20, Min. at Berlin, Breda, Göttingen, Freiberg, Leipsic, Marburg and Munich.

18. 1837. July 29. 8 55, Max. at Petersburgh.
9 0, Max. at Upsala, Copenhagen, Berlin and Breslau.
9 5, Max. at Göttingen, Leipsic, Freiberg, Marburg, Munich and Milan.

19. 1837. July 29. 11 40, Max. at Upsala, Berlin, Breda, Göttingen, Breslau, Leipsic, Freiberg, Marburg, Munich and Milan.

20. 1837. July 29. 12 10, Min. at Petersburgh, Copenhagen, Berlin, Breda, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.

21. 1837. July 29. 14 25, Min. at Breda.
14 25 to 30, Min. at Göttingen.
14 30, Min. at Petersburgh, Berlin, Breslau, Leipsic, Marburg, Munich and Milan.

22. 1837. Aug. 31. 6 35, Max. at Upsala, Berlin, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.
6 45, Max. at Dublin.

23. 1837. Aug. 31. 8 55, Min. at Upsala.
9 0, Min. at Berlin, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.
9 5, Min. at Dublin.

24. 1837. Aug. 31. 9 20, Max. at Upsala.
9 25, Max. at Berlin, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.
9 30, Max. at Dublin.

25. 1837. Aug. 31. 9 50, Min. at Upsala, Berlin, Göttingen, Breslau, Leipsic, Marburg and Munich.
9 55, Min. at Dublin and Milan.

26. 1837. Aug. 31. 10 25, Max. at Upsala, Berlin, Göttingen, Breslau and Leipsic.
10 30, Max. at Dublin, Marburg, Munich and Milan.

27. 1837. Aug. 31. 17 45, Max. at Upsala, Berlin, Göttingen, Breslau, Leipsic, Marburg, Munich and Milan.
17 50, Max. at Dublin.

28. 1837. Aug. 31. 19 5, Max. at Upsala.
19 10, Max. at Berlin, Göttingen, Breslau, Leipsic, Marburg and Munich.
19 15, Max. at Dublin and Milan.

29. 1837. Sep. 30. 3 50, Max. at Upsala, Copenhagen, Berlin, Göttingen, Breslau, Leipsic, Marburg and Milan.

30. 1837. Sep. 30. 7 30, Max. at Upsala, Copenhagen, Berlin, Göttingen, Breslau, Leipsic, Marburg and Milan.
7 35, Max. at Breda.

31. 1837. Sep. 30. 10 45, Min. at Upsala, Copenhagen, Berlin, Breda, Göttingen, Breslau, Marburg and Milan.

32. 1837. Sep. 30. 12 35, Min. at Upsala, Copenhagen, Berlin, Breda, Göttingen, Breslau, Leipsic, Marburg and Milan.

33. 1837, Sep. 30. 14 10, Max. at Upsala, Copenhagen, Berlin, Breda, Göttingen, Leipsic, Marburg and Milan.

34. 1837. Nov. 13. 6 25, Max. at Petersburgh, Upsala, Stockholm, Copenhagen, Berlin, Göttingen, Breslau, Leipsic, Freiberg and Marburg.
6 35, Max. at Munich.
6 40, Max. at Dublin.

35. 1837. Nov. 13. 8 50, Min. at Petersburgh, Upsala and Stockholm.
9 5, Min. at Copenhagen, Berlin, Breda, Göttingen, Breslau, Freiberg, Leipsic and Marburg.
9 10, Min. at Milan.
9 15, Min. at Dublin and Munich.

36. 1837. Nov. 13. 9 20, Max. at Petersburgh, Upsala and Stockholm.
9 25, Max. at Copenhagen, Berlin, Breda, Göttingen, Breslau, Freiberg, Leipsic and Marburg.
9 30, Max. at Dublin.
9 35, Max. at Munich.

		d	h m	
37.	1837.	Nov. 13.	10 25,	Max. at Upsala, Stockholm, Copenhagen, Berlin, Breda, Göttingen, Leipsic, Breslau, Freiberg, Marburg and Milan.
			10 30,	Max. at Dublin.
			10 35,	Max. at Munich.
			10 35 to 40,	Max. at Petersburgh.
38.	1837.	Nov. 13.	11 25,	Min. at Breda.
			11 25–30,	Min. at Dublin.
			11 35,	Min. at Berlin, Göttingen, Freiberg, Leipsic and Milan.
			11 40,	Min. at Upsala.
			11 45,	Min. at Petersburgh, Stockholm, Copenhagen and Munich.
39.	1838.	Jan. 27.	7 35,	Max. at Upsala, Copenhagen, Berlin, Breda, Göttingen, Breslau, Leipsic and Milan.
			7 40,	Max. at Marburg and Munich.
40.	1838.	Mar. 31.	23 20,	Min. at Upsala, Copenhagen, Berlin, Breda, Göttingen, Breslau, Marburg, Munich and Milan.
41.	1838.	Nov. 24.	7 35,	Min. at Upsala, Berlin, Göttingen, Breslau, Leipsic and Milan.
			7 40,	Min. at Breda and Munich.
42.	1838.	Nov. 24.	8 5,	Max. at Upsala and Breslau.
			8 10,	Max. at Seeburg, Breda, Göttingen, Leipsic, Marburg, Munich and Milan.
43.	1839.	Feb. 22.	13 45 to 50,	Min. at Breda.
			13 50,	Min. at Greenwich and Munich.
			13 55,	Min. at Berlin, Göttingen, Marburg and Milan.
			14 0,	Min. at Breslau, Leipsic and Heidelberg.
			14 5,	Min. at Upsala.
44.	1839.	Aug. 30.	10 20,	Min. at Upsala, Copenhagen, Berlin, Breda, Göttingen, Breslau, Leipsic, Marburg, Prague, Kremsmunster, Munich and Milan.
45.	1839.	Aug. 30.	10 35,	Max. at Upsala.
			10 35 to 40,	Max. at Breda.
			10 40,	Max. at Copenhagen, Berlin, Breslau, Leipsic, Prague, Kremsmunster and Munich.
			10 45,	Max. at Göttingen, Marburg and Milan.
46.	1839.	Aug. 30.	10 55,	Min. at Upsala.
			11 0,	Min. at Copenhagen, Berlin, Breda and Breslau.
			11 5,	Min. at Göttingen, Leipsic, Marburg, Prague, Kremsmunster, Munich and Milan.
47.	1839.	Aug. 30.	11 20,	Max. at Upsala.
			11 20 to 25,	Max. at Breda.
			11 25,	Max. at Copenhagen, Berlin, Göttingen, Breslau, Leipsic, Marburg, Prague, Kremsmunster, Munich and Milan.
48.	1839.	Aug. 30.	16 45,	Min. at Breda.
			16 50,	Min. at Upsala, Copenhagen, Berlin, Prague and Munich.
			16 50 to 55,	Min. at Göttingen.
			16 55,	Min. at Leipsic, Breslau and Marburg.
49.	1839.	Aug. 30.	17 5 to 10,	Max. at Breda.
			17 15,	Max. at Upsala, Berlin, Göttingen, Breslau, Leipsic, Prague, Marburg, Munich and Milan.
50.	1839.	Aug. 30.	18 30,	Min. at Breda.
			18 35,	Min. at Munich.
			18 40,	Min. at Upsala, Copenhagen, Berlin, Göttingen, Breslau, Leipsic, Prague, Kremsmunster and Milan.
			18 45,	Min. at Marburg.
51.	1839.	Aug. 30.	20 0,	Max. at Breda.
			20 5,	Max. at Munich.
			20 10,	Max. at Upsala, Copenhagen, Berlin, Göttingen, Leipsic, Breslau, Marburg, Prague, Kremsmunster and Milan.
52.	1839.	Aug. 31.	8 20,	Max. at Breda.
			8 25,	Max. at Upsala.

		d	*h m*	
	1839.	Aug. 31.	8 30,	Max. at Copenhagen, Berlin, Göttingen, Leipsic, Breslau, Marburg, Prague, Kremsmunster, Munich and Milan.
53.	1839.	Nov. 30.	6 25,	Min. at Upsala.
			6 30,	Min. at Copenhagen, Seeburg, Berlin, Göttingen, Breslau, Leipsic, Marburg, Prague, Kremsmunster and Milan.
			6 35,	Min. at Dublin.
			6 40,	Min. at Breda.
54.	1840.	May 29.	10 20,	Min. at Upsala.
			10 25,	Min. at Copenhagen, Berlin, Breda, Göttingen, Leipsic, Breslau, Brussels, Cracow, Kremsmunster, Marburg and Milan.
			10 30,	Min. at Petersburgh and Prague.
55.	1840.	May 29.	11 10,	Min. at Petersburgh.
			11 20,	Min. at Upsala.
			11 25,	Min. at Copenhagen, Berlin, Breda, Göttingen, Leipsic, Breslau, Marburg, Cracow, Kremsmunster and Milan.
			11 30,	Min. at Greenwich, Brussels and Prague.
56.	1840.	May 29.	11 30,	Max. at Petersburgh.
			11 40,	Max. at Copenhagen, Breslau and Cracow.
			11 40 to 45,	Max. at Breda.
			11 45,	Max. at Göttingen, Leipsic, Brussels, Kremsmunster, Marburg and Milan.
			11 50,	Max. at Greenwich.
57.	1840.	May 29.	13 30,	Max. at Petersburgh.
			13 30 to 35,	Max. at Breda.
			13 35,	Max. at Upsala, Copenhagen, Göttingen, Breslau and Cracow.
			13 35 to 40,	Max. at Leipsic.
			13 40,	Max. at Berlin, Greenwich, Brussels, Prague, Kremsmunster and Milan.
58.	1840.	May 29.	16 10 to 15,	Max. at Breda.
			16 15,	Max. at Upsala, Copenhagen, Berlin, Göttingen, Leipsic, Breslau, Brussels, Cracow, Kremsmunster, Marburg and Milan.
			16 20,	Max. at Petersburgh.
59.	1840.	May 29.	16 45,	Min. at Petersburgh, Copenhagen and Breda.
			16 45–50,	Min. at Brussels.
			16 50,	Min. at Berlin, Greenwich, Göttingen, Leipsic, Breslau, Cracow and Kremsmunster.
			16 55,	Min. at Prague.
60.	1840.	Aug. 28.	10 30,	Min. at Petersburgh, Upsala, Copenhagen, Berlin, Göttingen, Breslau, Kremsmunster and Milan.
			10 30 to 35,	Min. at Leipsic and Prague.
			10 40,	Min. at Dublin, Breda and Brussels.
			10 45,	Min. at Greenwich.
61.	1840.	Aug. 28.	14 25,	Min. at Petersburgh and Upsala.
			14 40,	Min. at Copenhagen.
			14 45,	Min. at Greenwich, Göttingen, Leipsic, Brussels, Marburg, Breslau, Prague, Cracow and Milan.
			14 50,	Min. at Breda.
			15 0,	Min. at Dublin.
62.	1840.	Aug. 28.	17 10,	Max. at Dublin.
			17 20,	Max. at Greenwich, Brussels, Marburg and Kremsmunster.
			17 20–30,	Max. at Göttingen and Prague.
			17 25,	Max. at Breda.
			17 30,	Max. at Petersburgh, Copenhagen, Berlin, Leipsic, Breslau, Cracow and Milan.
			17 30–35,	Max. at Upsala.
63.	1840.	Aug. 28.	18 55,	Min. at Petersburgh, Upsala, Berlin, Göttingen, Breslau, Cracow, Marburg and Kremsmunster.
			19 0,	Min. at Dublin, Greenwich, Copenhagen, Breda, Leipsic, Brussels, Prague and Milan.

d h m
64. 1840. Nov. 28. 0 50, Min. at Upsala, Stockholm, Copenhagen, Dublin, Greenwich, Berlin, Breda, Brussels, Göttingen, Leipsic, Breslau, Prague, Marburg, Kremsmunster, Cracow and Milan.
0 55, Min. at Petersburgh.
65. 1840. Nov. 28. 2 55, Min. at Petersburgh.
3 10, Min. at Upsala, Stockholm, Copenhagen and Breslau.
3 15, Min. at Dublin, Greenwich, Berlin, Breda, Brussels, Göttingen, Leipsic, Marburg, Kremsmunster and Milan.
66. 1840. Nov. 28. 6 40, Max. at Petersburgh, Stockholm and Copenhagen.
6 45, Max. at Berlin, Breda, Göttingen, Leipsic, Breslau, Prague, Cracow, Kremsmunster, and Milan.
6 50, Max. at Dublin, Greenwich and Marburg.
67. 1840. Nov. 28. 9 45, Min. at Petersburg, Upsala, Stockholm, Copenhagen, Dublin, Berlin, Greenwich, Breda, Brussels, Göttingen, Leipsic, Breslau, Prague, Marburg, Kremsmunster, Cracow and Milan.
68. 1841. Feb. 26. 12 45, Min. at Petersburgh.
12 50, Min. at Upsala, Stockholm and Copenhagen.
12 50 to 13 0, Min. at Göttingen.
13 0, Min. at Breda, Leipsic, Berlin, Marburg, Prague, Kremsmunster and Milan.
13 5, Min. at Breslau, Brussels and Geneva.
69. 1841. Feb. 26. 13 25, Max. at Upsala and Stockholm.
13 30, Max. at Copenhagen.
13 35, Max. at Berlin, Breda, Göttingen, Leipsic, Marburg, Prague and Cracow.
13 40, Max. at Breslau, Brussels and Geneva.
13 45, Max. at Milan.
70. 1841. Feb. 26. 15 35 to 40, Min. at Breda.
15 40 to 45, Min. at Göttingen, Geneva and Milan.
15 45, Min. at Petersburgh, Upsala, Stockholm, Copenhagen, Brussels, Berlin, Leipsic, Breslau, Marburgh, Prague, Kremsmunster and Cracow.
71. 1841. Feb. 27. 5 15, Max. at Petersburgh.
5 25, Max. at Upsala.
5 30, Max. at Copenhagen, Berlin, Göttingen, Leipsic, Breslau and Prague.
5 35, Max. at Breda, Brussels, Geneva and Milan.
72. 1841. May 28. 14 5 to 10, Max. at Breda and Kremsmunster.
14 10, Max. at Upsala, Stockholm, Christiania, Copenhagen, Dublin, Göttingen, Leipsic, Breslau, Brussels, Prague, Marburg, Cracow and Milan.
73. 1841. Aug. 27. 10 45, Max. at Petersburgh.
10 55, Max. at Stockholm.
11 0, Max. at Upsala and Christiania.
11 5, Max. at Copenhagen, Breda, Berlin, Göttingen, Leipsic, Breslau, Prague, Cracow, Kremsmunster, Geneva and Milan.
74. 1841. Aug. 27. 12 40, Min. at Christiania.
12 45, Min. at Upsala, Stockholm, and Copenhagen.
12 50, Min. at Petersburgh, Berlin, Göttingen, Leipsic, Breslau and Cracow.
12 55, Min. at Makerstoun, Breda, Prague and Kremsmunster.
75. 1841. Aug. 27. 13 10, Max. at Petersburgh, Upsala, Stockholm, Christiania, Copenhagen, Makerstoun, Berlin, Breda, Göttingen, Leipsic, Breslau, Prague, Cracow, Geneva and Milan.
76. 1841. Nov. 27. 8 40, Max. at Petersburgh and Stockholm.
8 45, Max. at Upsala, Christiania, Berlin, Breda, Göttingen, Leipsic, Breslau, Prague and Cracow.
8 50, Max. at Makerstoun and Brussels.

The following table shows the latitude and longitude of the places mentioned in the preceding catalogue.

Station.	Latitude.	Longitude from Greenwich.	Station	Latitude.	Longitude from Greenwich.
Petersburgh,	59° 56′	30° 18′ E.	Leipsic,	51° 20′	12° 22′ E.
Christiania,	59 54	10 44	Breslau,	51 6	17 2
Upsala,	59 51	17 38	Freiberg,	50 55	13 20
Stockholm,	59 20	18 4	Brussels,	50 51	4 22
Copenhagen,	55 40	12 35 E.	Marburg,	50 48	8 41
Makerstoun,	55 36	2 31 W.	Prague,	50 5	14 25
Seeburg,	53 56	20 45 E.	Cracow,	50 3	19 58
Altona,	53 32	9 56 E.	Heidelberg,	49 28	8 42
Dublin,	53 23	6 20 W.	Augsburg,	48 21	10 53
Berlin,	52 30	13 24 E.	Munich,	48 8	11 37
Hague,	52 4	4 19	Kremsmünster,	48 3	14 8
Breda,	51 35	4 47	Geneva,	46 11	6 9
Göttingen,	51 31	9 57	Milan,	45 28	9 12 E.
Greenwich,	51 28	0 0			

The following table shows for each station in how many cases the maximum deviation of the magnetic needle occurred earlier than at Göttingen; in how many cases it occurred at the same instant as at Göttingen; and in how many cases it occurred later than at Göttingen.

Maximum deviation of the magnetic needle.

	Earlier.	Simultaneous.	Later.		Earlier.	Simultaneous.	Later.
Petersburgh,	9	3	2	Munich,	3	20	6
Christiania,	1	3	0	Kremsmunster,	1	9	1
Upsala,	13	27	1	Hague,	0	6	1
Stockholm,	5	4	0	Breda,	4	21	2
Copenhagen,	7	17	0	Brussels,	0	4	4
Seeburg,	0	1	0	Marburg,	0	31	3
Altona,	0	3	0	Augsburg,	0	3	0
Berlin,	3	36	1	Geneva,	0	2	2
Leipsic,	2	39	1	Milan,	0	30	7
Breslau,	4	34	1	Makerstoun,	0	1	1
Freiberg,	0	9	0	Dublin,	1	1	9
Cracow,	1	9	0	Greenwich,	0	1	3
Prague,	1	12	1				

From this table we perceive that at most of the stations, the maximum deviation generally occurred simultaneously; that is, within a period of five minutes, for this is the interval of time between the observations. But at some of the stations the maximum generally occurred earlier than at Göttingen, while at others it generally occurred later than at Göttingen. If we draw through Göttingen a great circle of the earth running from N. 60° W. to S. 60° E., it will divide the stations in such a manner, that at all those on the N.E. side of this line, the maximum occurs earlier more frequently than later; while at all those on the S.W. side of it, the maximum occurs later more frequently than earlier. We may then conclude that the maximum deviation of the magnetic needle advances progressively like a wave over the earth's surface; and that the direction of its motion is nearly from N.E. to S.W.

The following table shows for each station in how many cases the minimum deviation of the magnetic needle occurred earlier than at Göttingen; in how many cases it occurred at the same instant as at Göttingen; and in how many cases it occurred later than at Göttingen.

Minimum deviation of the magnetic needle.

	Earlier.	Simultaneous.	Later.		Earlier.	Simultaneous.	Later.
Petersburgh,	7	7	3	Heidelberg,	0	0	1
Christiania,	1	0	0	Munich,	3	12	3
Upsala,	11	17	2	Kremsmunster,	0	14	1
Stockholm,	3	4	1	Hague,	1	2	0
Copenhagen,	6	16	2	Breda,	5	15	6
Seeburg,	0	1	0	Brussels,	0	7	4
Altona,	0	1	0	Marburg,	0	26	1
Berlin,	1	31	0	Augsburg,	0	1	0
Leipsic,	1	28	2	Geneva,	0	1	1
Breslau,	3	26	2	Milan,	0	24	4
Freiberg,	0	4	0	Makerstoun,	0	0	1
Cracow,	0	9	0	Dublin,	1	3	7
Prague,	0	11	5	Greenwich,	1	5	3

We perceive from these observations that the progress of the magnetic minima was nearly in the same direction as that of the magnetic maxima. We may draw a great circle through Göttingen in such a manner that at every station on the N.E. side of this line, the minimum occurs earlier more frequently than later; while at all those on the S.W. side of this line (without any important exception), the minimum occurs later more frequently than earlier. This line runs from N. 62° W. to S. 62° E., indicating progress in a direction from N. 28° E. to S. 28° W.

We thus see that the average progress of the maxima and minima was very nearly in the same direction; and if the average direction for either of these classes of waves is constant, it is probably the same for both of them. We may therefore combine both maxima and minima in the same table, and we shall obtain the following result.

Extreme deviations of the magnetic needle.

	Earlier.	Simultaneous.	Later.		Earlier.	Simultaneous.	Later.
Petersburgh,	16	10	5	Heidelberg,	0	0	1
Christiania,	2	3	0	Munich,	6	32	9
Upsala,	24	44	3	Kremsmunster,	1	23	2
Stockholm,	8	8	1	Hague,	1	8	1
Copenhagen,	13	38	2	Breda,	9	36	8
Seeburg,	0	2	0	Brussels,	0	11	8
Altona,	0	4	0	Marburg,	0	57	4
Berlin,	4	67	1	Augsburg,	0	4	0
Leipsic,	3	67	3	Geneva,	0	3	3
Breslau,	7	60	8	Milan,	0	54	11
Freiberg,	0	13	0	Makerstoun,	0	1	2
Cracow,	1	18	0	Dublin,	2	4	16
Prague,	1	23	6	Greenwich,	1	6	6

It is not improbable that the line which divides the stations at which the extreme deviations of the magnetic needle generally occurred earlier than at Göttingen, from those stations at which the extremes generally occurred later than at Göttingen, differs considerably from a great circle of the earth; but if we regard it as an arc of a great circle, then its direction must be from about N. 62° W. to S. 62° E., indicating a progress of the electric wave from N. 28° E. to S. 28° W.

It was stated on page 326 that Mr. C. V. Walker, from a discussion of the observations on the lines of telegraph in England, has arrived at the conclusion that in the S.E. part of England there is a stream of electricity drifting across the country from N. 42° E. to S. 42° W. We have now found that the irregular deflections of the magnetic needle, which are so remarkable during auroral displays, do not occur everywhere simultaneously, but are generally propagated over the surface of Europe in a direction from N. 28° E. to S. 28° W. It is possible that a more extended series of observations would show that these two directions are identically the same; but it is not improbable that the direction in England is somewhat different from that in Central Europe.

The time of greatest and least deflection at Dublin is on an average five minutes later than at Göttingen. Now Dublin is situated 222 miles from the great circle above mentioned, passing through Göttingen, indicating a progress of the electric wave of about 2700 miles per hour. The time of the extreme deviations at Upsala is on an average three and one-third minutes earlier than at Göttingen; while Upsala is situated 644 miles from the great circle above mentioned, indicating a progress of the electric wave equal to 11,000 miles per hour. If we make a like comparison for each of the other stations, we shall obtain velocities very unequal in amount. We thus perceive the difficulty of determining the average rate of progress of the electric wave. Sometimes the observations may be explained by supposing a single broad current of electricity flowing over Europe from N.E. to S.W. as in the case of Nos. 10, 23, 24, 26, 28, 36, 46, 65, 69, 71, etc. Occasionally the progress appears to be mainly from S.W. to N.E., as in the case of Nos. 58 and 70.

At other times the effect takes place simultaneously from Upsala to Milan, or at least within a period of five minutes, as in the case of Nos. 5, 7, 9, 11, 12, 13, 14, 19, 20, 29, 31, etc.

At other times it seems necessary to admit the existence of several currents moving in different directions, and probably with unequal velocities, as in the case of Nos. 15, 37, 38, 43, 45, 50, 51, 54, 57, 62, 74, etc.

Of the seventy-six cases of magnetic disturbance contained in the preceding catalogue, thirty-three occurred on days when an aurora was recorded at some one of the stations. Some of the deflections of the magnetic needle here recorded, were caused by the electric currents which prevail during the presence of auroras; while others occurred when no aurora was noticed. During the presence of an aurora, the magnetic deflections are greater than when there is no aurora; but they all seem to follow the same law of progress, with perhaps this exception, that during auroras there is an unusual number of cases in which there is the appearance of several currents moving simultaneously in different directions. The following is the list of auroras corresponding to dates in the catalogue.

1837. Aug. 31. 10^h P. M. Christiania. Slight aurora.
1837. Nov. 12. $6\frac{1}{2}^h$. England. Bright aurora with streamers reaching to the zenith.
Nov. 12–13. Brilliant aurora of a reddish color seen throughout France.
Nov. 13. England. Rain.
Nov. 14. England. Broad patches and streamers of a fiery red color.
Nov. 14. $11\frac{1}{2}^h$–$12\frac{1}{2}^h$. Christiania. Aurora of an intense crimson color.
1838. Jan. 28. $6\frac{1}{2}^h$–10^h. St. Petersburgh. Aurora.
1838. March 30. $9\frac{1}{2}^h$–10^h. St. Petersburgh. Aurora.
1838. Nov. 24. 10^h. Christiania. A flaming auroral arch about 10° altitude.
1839. Feb. 21. $6\frac{1}{2}^h$. Christiania. Aurora radiating towards the zenith.
1839. Aug. 30. 8^h–9^h. St. Petersburgh. Aurora.
1840. Aug. 28. 10^h. Christiania. Slight aurora.
1841. Aug. 27. 9^h–12^h. Christiania. Slight aurora.

During the aurora of Sept. 2, 1859, the disturbance of the magnetic needle was very great at Toronto, Greenwich, Brussels, Paris, Rome, Christiania, St. Petersburg, Catherinenburg, Nertchinsk, and Barnaul, but the observations are not reported with sufficient frequency to enable us to trace satisfactorily the progress of any single wave.

At Rome the greatest easterly deflection of the needle is said to have taken place Sept. 1st, at 7^h 20^m A. M. Göttingen time. At Petersburgh it took place at 7^h 48^m A. M. Göttingen time; and at Catherinenburg, Nertchinsk and Barnaul, it certainly took place within an hour of the same instant; it being impossible to determine the coincidence more closely, for the observations at these three places are only given at intervals of one hour.

New Haven, September, 1861.

Art. XXXVII.—*On the evidence furnished by Photography as to the nature of the markings on the Pleurosigma Angulatum;* by Prof. O. N. Rood, of Troy, New York.

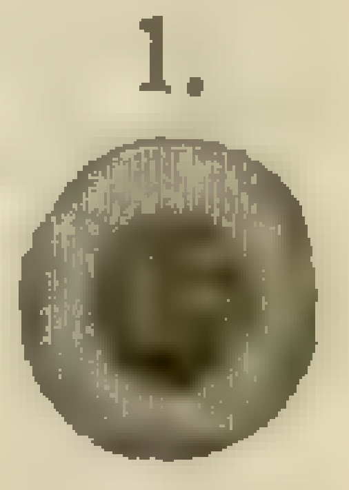
It is well known that the delicate markings on this infusorial shell have been diligently studied by microscopists for more than ten years, still the form of the dots is made a subject of dispute, one party maintaining it to be hexagonal, while their antagonists insist that it is circular. Photography has been called upon to aid in a settlement, and photographs of this shell were obtained by Mr. Whenam, which distinctly exhibited the *hexagonal* structure.

Without taking any particular interest in the controversy, I have devoted some time to photographing this test object, and have lately succeeded in obtaining, by the method indicated in my article in the September No. of this Journal, a fine, intense, sharp negative, magnified 1000 diameters.

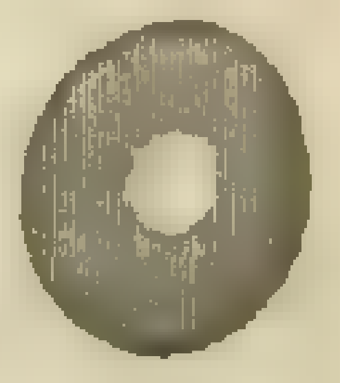
Almost the whole shell was nearly in focus, a large portion being in very accurate focus. When this negative was examined by the microscope, the dots were seen to be distinctly *circular*, and well-defined; by the aid of photography an enlarged and sharp negative was obtained, (magnified 7000 diameters) which yielded good prints on paper, showing the markings to be circular.

The dots in these negatives were not uniformly shaded but presented an appearance like that seen in the diagram, fig. 1. Furthermore, although the interior dark circle seemed to the eye uniformly dark, this was not actually the case, as was demonstrated by copying the negative with the camera and varying the time of exposure, when it became evident that the deposit of silver grew regularly denser from the circumference to the centre of these little circles; thus in this process positive prints on glass were obtained by the camera presenting the appearance roughly indicated in fig. 2.

1.



2.



Mr. Whenam states, that he has ascertained by a $\frac{1}{50}$, that the markings on this test object are due to spherical particles of quartz; it would appear that the results here detailed favor this view.

As will readily be comprehended by photographers, the delicate *shading*, which has been described, though very distinct on glass, *cannot be seen in prints on paper*, for exactly the same reason that the delicate shades in ambrotypes are lost, when an attempt is made to obtain prints on paper from them.

The production of *hexagonal* markings, which are often obtained, is probably due to a slight defect in the focal adjustment, as well as to a slight inequality in the oblique annular illumination.

The following is an instructive experiment: in a plate of brass a large number of circular holes are drilled, their disposition, &c. being similar to that of the circular dots on the shell in question; the plate is then carefully blackened. In my experiment the plate was a copy of an enlarged negative. The plate thus patiently prepared is placed before a bright light and the photographic camera leveled at it, a distance being selected such that small images of the circular holes are obtained. The full aperture of the lens should be employed.

By varying the focal adjustment of the camera slightly, these true circles become converted into tolerable hexagons, rhombs, or triangles, the latter being less distinctly marked than the two former.

Also, by altering the focus, an apparent reversal of the lights and shades takes place, the positions of the bright circular dots being seemingly occupied by dark dots; this is caused by the expansion of the thicker portions of the dark spaces between the circles, and so far as I can judge, with the means at my command, the reversal of the lights and shades in the dots on this test object examined by the microscope, under the imperfect focal adjustment, is due to the same cause, and is more apparent than real.

As it is impracticable to publish prints from these negatives in this Journal, samples will be sent, on application by letter, to those who may be particularly interested in this subject.

Troy, September 7th, 1861.

ART. XXXVIII.—*Waterglass;* by J. M. ORDWAY.

(Continued from page 165.)

PART II.

Properties.—Like some other substances whose constituents unite in a series of proportions, the alkaline silicates are rather trying to those who would square everything to exact atomic ratios. There are no fixed points certainly known by which we may make out a set of exact combinations, the intermediate compounds being considered as mixtures. Hence, though it is sometimes convenient to speak of sesquisilicates, bisilicates, and so on, these terms are to be looked upon as only approximative and analogical. It may be allowable, in order to convey to the mind a more definite idea of all such products of indefinite union, to express their composition in equivalents rather than in percentages of the constituents, and yet refrain from fanciful groupings into rational formulas.

Fused waterglass is little acted on by cold water, but when pure it dissolves without much difficulty in water kept continually boiling; though the solution goes on slowly and so evenly that bits retain to the last their exact original form, the sharp angles even not being rounded. So gradual is the diminution of the particles that one unaccustomed to the article would suppose, after a few minutes boiling, that solution was not taking place. In fact, an expert chemist once reported a certain sample of pure bisilicate of soda to be insoluble. Yet I found that forty-five minutes boiling of the same silicate, in the same state of division, caused the last particles to disappear.

When solutions are wanted day after day, it saves much time and fuel not to try to dissolve completely any given quantity in a particular amount of water, but always to keep an excess of the coarsely ground silicate in the kettle and boil, with frequent stirring, till the liquor acquires a sufficient strength as shown by the hydrometer. If the solution is dipped out hot, either the kettle must be immediately filled again with hot water, or the glass also must be taken out and held back till the water becomes hot, otherwise the silicate is apt to stick together and adhere most obstinately to the bottom of the kettle.

When a waterglass contains a greater proportion of silica than is needed to constitute a bisilicate, its solution is the work of many hours. And silicate of soda is somewhat harder to dissolve than a corresponding silicate of potash. As the relative amount of silica is increased, the precise point at which the product ceases to be integrally soluble, has never yet been ascertained. Fuchs speaks of his original potash glass,—$\dot{K}_2\ddot{S}i_5$,—as being entirely soluble. A monosilicate is readily taken up by water at less than the boiling heat.

As was before mentioned, the solubility of any silicate is much impaired by the presence of earthy impurities; and hence a sand which contains clay, mica, feldspar, lime, or oxyd of iron, is unsuitable for the manufacture of waterglass. When a product so contaminated is treated with boiling water, the earths and metallic oxyds are mostly left behind as compound silicates; and if sulphids are present, a part of the iron remains in the state of sulphuret, giving a blackish color to the sediment. But these foreign matters are not left wholly undissolved, for waterglass has the power of taking up small quantities of most oxyds, and the solvent power increases with the strength of the solution; so that a liquor which is slightly turbid while weak, may become quite clear by concentration, and on the other hand this clear strong liquid is rendered turbid again by large dilution. If iron is contained in the original materials, or if the melted mass is stirred with iron tools, or if the solution is effected in an iron kettle, iron can be detected in the perfectly transparent filtered liquid; and I have never yet succeeded in finding any means of getting entirely rid of this contamination. Changing protoxyd into peroxyd is of no avail, and an alkaline sulphid throws down only a part of it. From dry silicate of soda colored with manganese, I have obtained a decidedly pink liquid by boiling with water and filtering.* When a few drops of a weak solution of a metallic salt are added to waterglass and the whole is well agitated, the precipitate first formed will mostly or entirely disappear. A liquid silicate thus takes up no inconsiderable amount of the oxyds of iron, zinc, manganese, tin, lead, copper, and mercury. With protosulphate of iron and silicate of soda well shaken in a bottle partly full of air, and afterwards filtered, I have obtained a very deep blue solution. Waterglass saturated in the same way with zinc, after a while deposits the zinc again, or even coagulates into a gelatinous mass. Zincate of soda mixed with cold silicate at first shows no change, but a precipitate forms very soon. In like manner aluminate and glucinate of soda speedily cause a precipitation. But manganate, stannate, and chromate produce no alteration. Bolley[*] observed some years ago that even lime, magnesia, and baryta are slightly soluble in waterglass.

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* Kuhlmann mentions a soluble double silicate of potash and manganese. "C'est une matière vitreuse d'un violet foncé, qui donne une dissolution brune." "L'oxide de cobalt se combine aussi, mais en plus petite quantité, avec le silicate de potasse." —*Silicatisation*, 3me edition, Paris, 1858, p. 17.

Thus we see that silica, though it has no neutralizing power, exhibits some reactions which go to vindicate its acid nature. But its chemical activity is more distinctly shown when earthy or metallic protoxyds in the state of hydrates are mixed, in equivalent quantities, with dissolved waterglass. In most such cases there is a speedy coagulation and the viscosity of the silicate is destroyed. The effect is very striking when we stir milk of lime into a tolerably strong solution of waterglass. The mixture almost immediately thickens or 'sets,' and it becomes crumbly, if the stirring is forcibly kept up. In solutions too dilute to set, the silicate of lime forms an exceedingly bulky precipitate. And here it may be remarked that the recommendations which have sometimes been made for recovering caustic potash or soda from the soluble silicates by adding lime, must have been given *a priori* and not after actual experiment. For the resulting silicate of lime is so voluminous and retentive of liquor, that the separation of the alkali by filtration or washing is utterly impracticable. The extraction can be effected only by drying down the mixture and then lixiviating the dehydrated mass.

Litharge, when ground exceedingly fine with water, coagulates a silicate, but does not entirely destroy the toughness, especially of silicate of potash.

The anhydrous oxyds of zinc, mercury, and copper may be mixed with waterglass without producing any apparent change, but there is reason to suppose that a union takes place in the course of time. The same is true of the hydrates of the sesquioxyds of iron, aluminium and chromium.

Of course most earthy and metallic salts effect a double decomposition when mixed with alkaline silicates, and generally a thickening of the whole mass very soon ensues. But sulphate and carbonate of baryta seem to be without action; and so does fluorid of calcium. Most of the lime salts however, whether soluble or insoluble, are particularly energetic in their operation.[†]

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[*] Kopp and Will's Jahresbericht for 1858, p 140.

[†] Pelouze says that even common glass on being boiled with sulphate of lime, yields no inconsiderable amount of sulphate of soda.—Leibig and Kopp's Jahresbericht for 1856,—p. 355.

Carbonate of lime does not produce any decided change, yet when it is boiled with a very silicious silicate it becomes flocculent, showing that there is a partial exchange of constituents.

The basic carbonates of zinc and magnesia, and the carbonates of lead and manganese produce an immediate coagulation. The carbonates of ammonia and the bicarbonates of potash and soda, by virtue of their weakly bound carbonic acid, cause an instantaneous thickening in strong solutions of the silicates.

A highly silicious waterglass gives precipitates with the alkaline carbonates, acetates, tartrates, phosphates, nitrates, sulphates, and chlorids, the exact nature of which has never yet been ascertained. But the bisilcates, as well as those still more alkaline, are not affected by carbonate, tartrate, nitrate, or sulphate of soda; while sesquisilicate of soda is troubled only by a very strong solution of chlorid of sodium. The precipitate with chlorid of sodium in any case appears to be a double combination of silicate and chlorid, which is insoluble in water, but is readily decomposed by an acid. It is opaque and very voluminous, though after being washed and dried it is found to amount to only a small proportion of the quantities mixed.

Ammonia salts throw down the silica from a strong solution of waterglass, while the ammonia is set free, silicate of ammonia being a compound apparently incapable of more than a momentary existence under the ordinary pressure of the atmosphere.

When a dilute acid is added to a weak solution of a silicate there is no immediate precipitation of silica, but after some hours the whole gelatinizes. With sulphuric acid the coagulation comes on soon; but with chlorhydric acid the change is delayed a long time, and the mixture may sometimes even be heated and partially evaporated and still remain liquid.

Alcohol and wood spirit precipitate the silicates as such, even when the solutions are very weak. Fuchs recommends as a good means of purifying potash waterglass and obtaining it in a nearly solid state, to mix a strong solution with one-fourth its volume of rectified spirit of wine. The precipitate is at first bulky and opaque, but by standing a day or two it contracts and becomes firm and transparent. He says further that "soda waterglass is not at once perfectly precipitated, like potash waterglass, by rectified spirit of wine, but is only changed to a slimy mass; if it is not perfectly saturated with silica and is somewhat diluted, it either gives no precipitate, or gives one only after some time, whereby it can be easily recognized and distinguished from potash waterglass." My own experiments on precipitation by alcohol,—the details of which must be reserved for a future paper,—show that this statement is much too broad. For silicate of soda is thrown down even more readily and completely than silicate of potash. The deposit is always softer, indeed, than that from a corresponding potash compound, but while it is sometimes in the liquid state, under other circumstances it may constitute a firm, almost solid mass. And on the other hand, a pretty alkaline potash silicate may be precipitated in the fluid form. These products usually contain about fifty per cent of water, and are somewhat less alkaline, and considerably freer from foreign salts than the silicates from which they have been derived. And in most cases, they dissolve readily in cold water.

Very few combinations of silica and alkalies have been found capable of crystallization. Fritzsche* obtained the sesquibasic silicate of soda in the form of rectangular prisms of the composition $\dot{Na}_3 \dddot{Si}_2 \dot{H}_{27}$; also the same salt under another form with 18 eqs. of water. And Yorke† mentions crystals containing 21 eqs. of water. Fremy‡ reports the singular compounds $\dot{K}_4 \dddot{Si}_3 + Aq.$ and $\dot{Na}_4 \dddot{Si}_3 \dot{H}_{26}$ to be easily crystallizable, and he thinks the latter was what Fritzsche really produced. Fremy§ also speaks of the soluble trisilicates,—$\dot{K}\dddot{Si}_3 + Aq.$ and $\dot{Na} \dddot{Si}_3 \dot{H}_{20}$,—as crystallizable. The details of his researches, however, have not been published, and we know not on what grounds he makes a statement so much at variance with what the character of the other acid silicates would lead us to expect. For nothing between the monosilicates and the trisilicates shows the slightest disposition to assume the crystalline form. On the contrary, as the relative proportion of acid is increased beyond a sesquibasic silicate, the product takes on an increasingly gummy or viscous character. And one of the most important properties of true waterglass is its adhesiveness,—a quality which it possesses in a degree not approached by any other inorganic substance. In this respect silicate of soda somewhat surpasses silicate of potash; for when the soda silicate is boiled down till it begins to adhere to the bottom of the dish, it still remains a strongly cohesive liquid. While silicate of potash similarly treated, is fluid and glutinous at first, but in the course of a day or two, becomes a slightly tenacious jelly. With the exception of this last trifling change, solutions of waterglass, of whatever strength they may be, if kept from the air and from frost, remain unaltered for years. When they are exposed to intense cold, a part of the water freezes, but reunites on thawing.

When a concentrated silicate is spread out so as to present a large surface to the air, it very gradually dries and becomes unimpressible, but does not part with all its water and acquire a stony hardness except as the alkali becomes carbonated. Fuchs found an air dried potash waterglass to contain twelve per cent of water. This must have been extended in very thin layers, for a stratum of bisilcate of potash, about a quarter of an inch in thickness, which I left on paper in a very dry loft for more than two years, still retained 29 per cent of water and was capable of bending under a steady, protracted pressure.

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* Berzelius, Traité de Chimie,—Ed. Française, 2e, iii, p. 233.

† Kopp and Will's Jahresbericht for 1857, p. 162.

‡ Comptes Rendus, xliii, p. 1148.

§ Id. p. 1147.

To expel the last portions of water directly, requires a heat approaching to redness; and the puffy, dehydrated mass is no longer entirely soluble in water. Fuchs attributes the passiveness of the silica in this case to the absorption of carbonic acid in drying down, and says that if the anhydrous mass is heated to full redness so as to decompose the carbonate, the solubility is restored. It is indeed true that when waterglass is desiccated in an open dish over a lamp, it cannot remain unaffected by the products of combustion, and the residue will effervesce with acids. There are also two other possible sources of carbonic acid. It may have been taken up from the air in first dissolving the silicate, or in preserving the liquid in imperfectly closed vessels. And again if the solution has not been made with perfectly pure water, or if it has been exposed to falling dust, it is likely to contain organic matter which on being strongly heated in the presence of an alkali, is resolved into the simplest products of decomposition.

All these possible chances of influence seem to have been disregarded by Lielegg,* in examining a liquid bisilicate of soda made at Munich. To determine the water, he kept some of the solution a long time in an air bath at a temperature between 90° and 100° C., and then slowly raised the heat to dull redness. On digesting the mass with warm water there was left a residue which proved to be silica, while a sesquisilicate,—$\dot{Na}_5\ \ddot{\bar{Si}}_7$,—was dissolved. He hence concludes that at a red heat bisilicate of soda cannot exist, but is resolved into silica and a salt of constant composition, a sesquisilicate. But the facts by no means justify his inference. Besides neglecting the effects of carbonic acid,—and drying in an air bath would give a full opportunity for absorption,—Lielegg fails to notice that the sesquisilicate itself relinquishes a part of its acid in parting with its water. It will hardly do to make the broad statement that bisilicate of soda cannot exist at a red heat, when a pure bisilicate made in the dry way, and of course cooled down through the "Glühhitze," is completely soluble; and it would be absurd to consider that which dissolves without any perceptible resolution into parts, as consisting of sesquisilicate and soluble silica. Taking everything into account, we are warranted in affirming only that when dissolved waterglass is rendered anhydrous by exposure to the necessary heat, a part of the silica goes over into the passive state. And I find this to be true even when especial pains are taken to preclude every source of carbonic acid. The reason remains to be discovered. Fremy† found that after the trisilicates had been carefully dried, water dissolves the alkali out of the residue, and leaves the silica, which last, according as the strength of the heat has been, is or is not soluble in dilute alkalies. The sesquisilicates also he proved to be decomposable by heat, but the monosilicates were ascertained to be unchanged by dehydratation.

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* Dingler's Polytech. Journal,—cliii, p. 49.

† Liebig and Kopp's Jahresbericht for 1856, p. 353.

Uses.—Numerous applications have been proposed for waterglass, and for many purposes it has really proved to be of permanent value, but the cases in which there are not material drawbacks to its employment, are very few. The properties which render it available in the arts, are:—

1. Its adhesiveness in the hydrated state. 2. Its vitrifying power in the dry state. 3. Its alkaline nature. 4. Its capability of yielding soluble silica. 5. Its peculiar chemical relations as a whole.

1. Adhesiveness. On account of this most striking characteristic of waterglass, its discoverer said that it might justly be called "mineral glue," and to a certain extent the similarity holds good. It differs however from glue, and most other cementing substances, in continuing to shrink after it has become apparently dry. A strong silicate of soda solution forms a good colorless cement for glass, porcelain, and stone, but when shut up in such impervious substances, it is very slow in becoming water proof, and as it does so, its strength is much impaired. For wood and other porous materials it does not answer, since they allow the access of air which, by its carbonic acid, decomposes the silicate and destroys its tenacity. I have tried silicate of soda in the laboratory for pasting labels on glass bottles. It does pretty well, only when it is once on, it will never wash off, though the paper itself may be removed by washing. It possesses no advantages over gum or flour paste, with the single exception of not being liable to mould by keeping.

I have often made use of a strong solution of waterglass mixed with clay and sand, for setting fire bricks. Such a mixture undergoes partial fusion in a strong heat, and makes very tight, firm joints. For brick work that is to be kept moderately hot, a mortar composed of the same ingredients, but with a larger proportion of the silicate, makes an excellent cement which continues hard and tough, while lime mortar exposed to heat, is apt to dry up and lose its binding power. Mr. Joseph D. Gould tells me that he has found fibrous asbestos wet with a strong silicate of soda liquor to make a most excellent packing for the joints of apparatus exposed to hot acid vapors.

When liquid waterglass, either by itself or mixed with an inert substance, is thinly spread out on any surface, it dries to a strongly adherent, hard, transparent varnish; but it still goes on absorbing carbonic acid from the air, and the residual silica being quite incapable of extension, becomes traversed with an infinity of minute cracks, so that the original smoothness and clearness are greatly diminished. Still the adhesion continues and the silicious coating can no longer be removed by washing with water. Hence the soluble silicates are adapted to fix various pigments. In fact this was the first use to which they were made subservient, and they have latterly been much employed in Europe for painting. When waterglass mixed with the lighter colors is applied to wood, the alkaline nature of the vehicle betrays itself by a softening and discoloration of the surface, which effects however are quite inconsiderable, if the wood is new and clean. Another difficulty is that the fixed coating having no elasticity, cannot accommodate itself, like an oily or resinous film, to the expansions and contractions of the wood in wet and dry weather, nor yet to slight inequalities of shrinkage. Still waterglass paint may do well in places where it is not exposed to alternations of dampness and dryness. And for outbuildings, fences, and bridges,—smoothness being of little importance,—a mixture of zinc white, chalk, ochre, or terra di Sienna with silicate of soda, might in many cases, be substituted for the easily detached lime washes so commonly used in this country.

Fuchs at first made and applied waterglass to render wood incapable of being inflamed, and experiments instituted by order of the British admiralty are said to have proved its efficacy in this respect.* Better antiphlogistics may have been found, but none of them are capable of serving at the same time as varnishes.

We are told by some that a silicate secures wood against the destructive effects of air and moisture, an assertion which savors a little of enthusiasm, and admits of a reasonable doubt. Since waterglass can do little toward permanently excluding air and moisture, and as little toward counteracting albuminous matters within the wood, its preservative power must be far inferior to that of oil, and no greater than that of lime whitewash. In estimating the value of silicate of potash or soda for any such use, we must not be deceived by the smooth, glassy, continuous, impenetrable coat which it forms at first. Its smoothness, its lustre, its continuity, its imperviousness are all destined to pass away by the slow action of the atmosphere. Indeed the value of waterglass in painting depends in some measure on its alterability by carbonic acid, since when first dried it is still soluble in water, and only after having been exposed to the air for several days, is it safe from moisture or rain. The fixation is sometimes hastened by brushing over the coating of silicate, after a day or two, with a weak solution of sal-ammoniac or of carbonate of ammonia.

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* **Répertoire de Chimie Appliquée, i, 63.**

Silicate paint, being itself unyielding, is much more suitable for unyielding surfaces, as for glass, stone, or brick, or for walls plastered with lime mortar. As it unites more intimately with such substances, it is less liable to scale off from them than from wood. But to guard against a loose adhesion, it is better in all cases, to paint with a very thin waterglass several times, allowing some days to elapse between the successive applications, rather than to use a strong solution once. The thinner silicate penetrates deeper and takes a surer hold. It is also sooner and more uniformly fixed by the absorption of carbonic acid.

Many pigments are incompatible with waterglass, and must be rejected. Such are white lead, Prussian blue, Schweinfurth green, and animal or vegetable colors. But there is still left a sufficient variety to select from, as we may take zinc white,* chalk, sulphate of baryta, yellow ochre, cadmium yellow, Venetian red, terra di Sienna, green oxyd of chrome, umber, ultramarine, lampblack, or bone black. It is said that chrome yellow and chrome red may also be used, but it would be safer to leave them out, as their chemical relations indicate their unsuitableness. Before applying colors ground up with waterglass, it is best to 'prime' the surface with simple silicate liquor, and let it stand 24 hours or more. This fills the pores and makes a ground to which the paint afterwards laid on, will adhere more firmly. A solution of silicate of soda for paint or for priming, should not exceed the sp. gr. 1·15 and it is better to take it much weaker. Much of the silicate of soda found in the market in this country, is unfit for paint. It is sometimes too alkaline, and is very often too much contaminated with foreign salts which are prone to crystallize and loosen the silica before it becomes properly fixed. A good silicate should be bright, transparent, homogeneous, and very light colored, and should show no special tendency to absorb moisture in a damp atmosphere. It is none the worse for requiring several hours boiling to bring it into solution, provided this refractoriness does not arise from earthy matter in combination.

Walls plastered with lime mortar, may be rendered very hard, close and smooth, as well as capable of being washed, by applying a few times a silicate, either alone, or mixed with chalk or any coloring material.

Since 1840 Fuchs has introduced a new plan of executing works of art on plastered walls, designating his method by the name of "stereochrome,"—*fast paint.* A basis is first prepared with a rather sandy lime mortar, and when this has taken up a due proportion of carbonic acid and become well set, the superficial glaze of carbonate is removed, by scouring with sandstone or by washing with weak phosphoric acid. The plaster is then repeatedly soaked with a dilute solution of very silicious waterglass, the silicate being allowed to dry and fix between the consecutive drenchings. The well saturated ground is now covered with a thin stratum of nicely prepared, but meagre, mortar, and this coat is treated in the same way as the basis. When the silicatization is finished, the surface should still be rough and absorbent. If it is glazed over so as to be too impervious, Fuchs recommends to open the pores by pouring on alcohol and burning it off. On the wall so prepared, the painting is executed with colors ground up with mere water. And finally the pigments are fixed by repeated affusions of a rather alkaline double silicate of potash and soda. To avoid any displacement of the colors, the fixing liquid is thrown on the first time, in the form of a fine spray, by means of a suitable syringe. As the artist must frequently change his palette, it is impracticable to use the paints mixed directly with waterglass, since the silicate would be always drying up, and the brushes would get stiff and hard. Brushes soaked with a silicate should never be allowed to dry without being first thoroughly washed with water.

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* In one or two standard works it is stated that zinc white sets rapidly with waterglass. But this is an error. The mixture may be kept for several days without any change.

Fuchs mentions as proofs of the excellence of the stereochrome, two paintings which had been exposed out of doors to all changes of weather, for six years, and still were as bright and as fresh looking as though they had just left the artist's hand.

Creuzburg* has resorted to a modification of Fuchs' method, for common painting, and thus makes use of pigments which would otherwise be inadmissible. To any surface he applies alternately thin waterglass and a mixture of the color with skimmed milk, till the requisite body is attained. He says that it is only necessary to allow one coat to dry before laying on another, but this would be hardly consistent with a solid fixation. A final varnishing with oil is recommended, to impart lustre and prevent saline efflorescence.

Creuzburg reckons as advantages of waterglass paint:—1. Rapidity of drying. 2. Freedom from smell. 3. Purity of tint. White oil paint is modified in tone by the color of the oil, and it is farther liable to get dingy by a chemical change which goes on in the dark. 4. Durability.† 5. Resistance to fire. 6. Cheapness.

When a silicious paint is to be laid on a metallic surface, it is well to have the metal moderately warm, otherwise the coating is liable to crack and scale off. For stoves that are sometimes red hot, it is said that a mixture of waterglass and peroxyd of manganese, makes a good blacking not liable to burn away.

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* Wagner's Jahresbericht, iii, 133,

† To illustrate the short durability of oil paint Creuzburg mentions the fact that a coat of white lead and oil, after having been exposed to the weather for two years or so, is so far changed that the white lead rubs off with the greatest ease. This however is rather an extreme case, and it is said by practical men to hold true more particularly with ceruse manufactured by the Dutch method. White oxychlorid of lead and zinc white are not considered liable to this disadvantage.

Kuhlmann has succeeded in printing wall paper with waterglass as a vehicle for the pigments. And he says that any paper printed with colors unalterable by alkalies, may be varnished with a silicate, after it has been pasted on the wall, and will afterwards bear rubbing and even washing.

Kuhlmann likewise recommends a mixture of waterglass with lampblack, ivory black, or vermilion, for a writing ink capable of resisting all destructive agents. But according to Baudrimont, after a writing executed with silicious ink, has been long exposed, the silicate having undergone decomposition, the letters may be easily erased.*

In 1840 Leykauf proposed the silicates for fixing ultramarine on cloth, thus substituting a very cheap material for the albumen and casein largely used in pigment printing.† But it can not be used for goods that are to be steamed, and in any case the alkali has to be removed by passing the printed cloth into an acid bath, which operating too rapidly must deprive the silica of most of its adhesive force. In fact though this plan was brought forward so long ago, a prize is still offered for the discovery of some cheap and efficient substitute for albumen.

The binding power of the silicates has been turned to account by Kuhlmann in the hardening or "silicatization" of soft porous stone. A tender material, like chalk, may be rendered available for building purposes by repeated saturations with waterglass. And the durability of many buildings already erected, is greatly enhanced by subjecting the outside surface to a similar treatment. The same process has been found efficacious in preserving some ancient statues freshly exhumed, which would otherwise have fallen to pieces after a short exposure to the air. Fragile palæontological specimens have also been strengthened and saved by silicatization.

Ransome says that merely washing stone with waterglass is not sufficient, as the silicate retains its solubility for a long time. He therefore thought fit to secure a patent for fixing the silica by the subsequent application of chlorid of calcium. But notwithstanding the strong commendations of interested parties, it would appear from recent discussions of the subject in London that this method has not proved entirely satisfactory. Indeed hasty fixation can hardly be compatible with tenacity and permanence, and it is probable that the best effect would be attained by using a silicate alone, many times and at distant intervals.

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* Liebig and Kopp's Jahresbericht for 1855, p. 869, note.

† As it takes 24 dozen eggs to furnish 1 kilogram of dry albumen, it has been computed that 330,000 hens are needed to produce the 125,000 kilos. of albumen consumed yearly in Alsace alone.—*Rep. Chimie, App.*—iii, p. 101.

Kuhlmann meets the difficulty by a plan theoretically perfect, but involving too much expense, which is to use silicate of potash and render both constituents insoluble with fluosilicic acid.

To impart a more agreeable color than some stones naturally possess, Kuhlmann impregnates them, first of all, with a salt of iron, manganese, copper, or chrome. The silicate afterwards applied precipitates the metallic oxyd within the pores and produces the desired tint, either at once or after the absorption of a farther portion of oxygen from the air.

With regard to stone, silicatization is of little importance in this country, as we have few varieties that need artificial hardening. But in some places there is a lack of good clay for brick making, and such earth as is worked, gives very tender, absorbent brick, ill calculated to bear handling and exposure to the weather. In many cases it would doubtless be advantageous to silicatize the exposed surface of the bricks after they have been laid. This is deserving of especial consideration where they are subjected to the action of sea water, which is particularly destructive of porous building materials.

In connection with painting and silicatization it should be observed that on surfaces charged with silicate of soda a whitish efflorescence of carbonate of soda, is likely to appear several times, but this can be removed as often as it forms, by gently washing with water, or out of doors the rain will carry it off.

Fuchs in his first memoir, showed the possibility of making an artificial stone with clay, sand, and a solution of silicate of soda. Such a mixture after being moulded into any desired form slowly dries to an exceedingly tough, resistant mass, but it is not waterproof. It hardens better and more uniformly when the drying is hastened by a moderate heat. A dull red heat dehydrates the silicates and renders the mass friable. It is doubtful whether an artificial stone that deserves the name, can be made without exposing the mixture, after moulding and drying, to a heat strong enough to revitrify the silicate. By such treatment the waterglass recovers its tenacity and, by forming a chemical union with the other ingredients, becomes truly insoluble. The baked ware should, of course, be slowly cooled so that the cementing material may become properly annealed.

In 1844, Ransome took out a patent in England for a stone prepared with *liquor silicum*, limestone powder, or chalk, and sand. His process is a rather expensive one, but the article produced is said to be of excellent quality. There is reason to believe that *liquor silicum*, is less suitable for such purposes than the far cheaper sesquisilicate or bisilicate of soda. In making trials in a small way I have been unable to get a good ware with common sand, but succeeded best with a mixture of crushed granite, chalk, or bone ashes, and strong sesquisilicate of soda. Unless a fair allowance of some absorbent substance,—like carbonate of lime, burnt bones, or roasted clay,—is used, the silicate dries first on the outside and forms an impervious varnish which prevents the escape of moisture from the interior. If we resort to a weaker solution of waterglass, this dries at the surface without closing the pores, but then the liquid inside is drawn outwards by capillary attraction, and the central parts are left too meagre while the exterior is too rich in vitrifiable matter. Ransome ingeniously obviates the difficulty by heating the moulded stones in a closed space, so that the surface is in contact only with an atmosphere saturated with moisture, while the interior is acquiring such a temperature that on opening the room most of the water will pass off rapidly and keep the pores open.

Wagenmann* succeeded in making artificial meerschaum by mixing waterglass, lime, magnesia, and carbonate of magnesia, and simply drying.

Considered merely with regard to its mechanical properties, waterglass would appear very suitable to replace starch and glue as a glazing material, in many cases. In fact it was brought forward by Leigh, a year or two ago, as a substitute for starch in sizing cotton yarn for weaving, and in putting the final finish on cotton fabrics. But its chemical character indicates its unfitness for such uses. Starch after drying remains unchanged itself, and has no action on the stuffs. While a silicate is altered by exposure to the air, and loses its smoothness as well as much of its rigidity. Besides this it is alkaline, and therefore tends to weaken the fibre,—an effect which becomes at once apparent when waterglass is used strong for producing very stiff fabrics.

It is common to increase the weight and apparent substance of flimsy cloth by passing it through starch to which fine clay, chalk, or sulphate of lime has been added. In place of this preparing material so easily removed by washing, Grüne proposed to pad the cloth in silicate of soda, and then run it through a weak acid liquor, so as to precipitate silica within the fibre and make a permanent stuffing. But it should be borne in mind that weak acid acting on a weak silicate, gives a silica soluble in water. A salt of zinc or magnesia would be a more suitable precipitant. The plan, however, even were it effectual, is too troublesome and costly. A factitious body can be of little use except to make the goods sell better; and it is hardly worth the while to go to much expense for a mere deception. Most manufacturers would prefer to cheat with clay that is worth only a cent a pound, rather than with a precipitated silica that could hardly cost less than three; and most consumers would rather have the three extra cents appropriated to extra cotton.

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* Wagner's Jahresbericht, ii, 118.

As a thick solution of waterglass dries at first to a glassy, impenetrable varnish, Grüne has suggested its employment as a resist in calico-printing;—that is, as a substance to cover those parts of the cloth that are to remain white, and prevent them from imbibing the dyeing liquor. But hot water dissolves the silicate rapidly, and hence such a resist can answer only when the printed cloth is to be immersed for a very short time in cold dyeing baths.

In the manufacture of porcelain wares, pastes are sometimes used that are devoid of plasticity and requires the addition of some glutinous material to give them sufficient cohesiveness to allow them to be moulded. For such pastes "mineral glue" would probably do better than a destructible mucilage.

2. In fusibility and fluxing power waterglass greatly resembles borax, and in some cases may well replace this expensive salt. The iron hoes employed in stirring the fused silicate during its preparation are kept remarkably clean and bright by the melted mass, and this naturally suggests its use in cleansing and protecting heated metallic surfaces. Indeed according to Wagner,* the double silicate of potash and soda,—which melts more readily than simple silicate,—forms a good substitute for borax in brazing and in welding. He rather injudiciously recommends the mixed materials instead of the ready formed glass.

Another application which has proved successful and has been patented in this country, is the manufacture of wick for snuffless candles. By passing the wickyarn through silicate of soda and then through acetate of lead, it is charged with enough silicate of lead to vitrify all the ash as the candle burns away.

Recently an English patent has been issued, for treating cloth and paper in the same way,—but with stronger solutions,—in order to render them uninflammable.

Leibl† long ago recommended a solution of waterglass as a glaze for pottery ware, to be applied before the burning. It has the advantage of being free from lead, and as it penetrates farther into the body of the ware than any flux insoluble in water, it should take a firmer hold. It is said however that Leibl's glaze has been tried with unfavorable results, but it is quite possible that the experiments were not properly conducted.

Though sulphate of soda is exceedingly cheap in this country, and sulphate of potash, abundantly obtained in refining pearlash, finds sale only at a very low price to alum makers, our glass manufacturers generally employ only the carbonates. I have long sought to produce purified alkaline silicates that might economically replace the carbonates in making the better qualities of glass, but have been met by a hitherto insurmountable difficulty. The alkaline sulphates can be decomposed by silica and coal much more easily and certainly in a reverberatory furnace than in a glass pot, and with sulphates freed from iron, pure silicates of potash and soda could be made with far greater facility than the carbonates. When a clean sulphate is converted into waterglass and this is dissolved, the few impurities derived from the furnace and from the fuel, are left behind. If we concentrate the clear solution and stir into it rapidly a thick, smooth milk of lime, the mixture soon sets and crumbles, and the whole mass can be dried down with ease. The resulting double silicate mixed with the requisite quantity of sand, fuses in less time than the ordinary glass mixture, and boils up but very little. Precipitated dicarbonate of zinc, ceruse, or litharge may be substituted for lime. The litharge mixture however toughens after coagulation, and is harder to reduce to dryness.

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* Wagner's Jahresbericht, iii, 137.

† Id., ii, p. 211.

But economy will not allow the purification of both the material and the silicate made from it. It is in using crude sulphate and applying the refining process to the waterglass only, that the difficulty above mentioned occurs. When iron is contained in the fused silicate, a little of it will enter into solution, and there has yet been found no means of throwing it down. This dissolved iron therefore is carried forward into the glass and imparts too much greenness to be overcome by any reasonable amount of correctives. A small quantity of arseniate of soda, oxyd of antimony, or stannic acid lessens the color, but does not entirely remove it.

It would seem therefore that the only available way of adapting waterglass to the glass manufacture, is to start with purified materials and make a product which can be directly fluxed with proper proportions of sand and lime or litharge.

Were it desirable to introduce baryta into glass, a suitable material in the form of a double silicate of baryta and potash or soda, could be easily prepared from the pure sulphates. But lime is much cheaper than baryta in any form, and as to toughness and brilliancy I have found on trial in the small way very little difference between a glass made with lime and one containing an equivalent amount of baryta. Neither does zinc glass possess much advantage, in any respect, over an equally pure lime glass.

Some years ago, with the idea of effecting a saving in pearlash, precipitated carbonate of baryta was largely used by some manufacturers of lead glass, the price being about half that of litharge. They did not stop to consider that one equivalent of lead and one of pure lime would go much farther than two equivalents of baryta in increasing the lustre and fusibility of glass, and add no more to the cost.

3. As a source of soluble silica waterglass may prove valuable in agriculture, and years ago it was proposed for a manure. Then its high price was a serious objection, but now it has become a

common article of manufacture, and competition has reduced the price to less than that of Peruvian guano. Indeed were there a large demand, a silicate of soda suitable for manure could be made from the sulphate and sold in this country for two cents a pound, and yet yield a liberal profit to the manufacturer.

If waterglass is used in solution, it ought to be applied to the growing crops by frequent waterings. But perhaps the better way would be to spread the finely ground, dry silicate mixed with other manures, and thus supply to the soil a material similar to its feldspathic ingredients, but far more susceptible of decomposition. One or two agricultural experiments do not suffice to establish any particular point, and we have as yet too few accounts of the effects produced by using silicate of soda, to enable us to decide as to its real practical value to the farmer.

There is another application in which waterglass acts as a purveyor of soluble silica. According to Kuhlmann common lime mortar may be rendered hydraulic by the addition of a few per cent of dry, pulverized silicate of potash or soda, and poor cements can, by the same means, be made equal to the best. A solution of waterglass will not answer the purpose so well, because it sets before the mortar can be got into its place. Still Kuhlmann recommends both the fine powder and the solution. He even attributes the peculiar character of hydraulic cements to the alkaline silicate naturally present in them, but his views have not been fully substantiated.

4. In the dissolved silicates we have an alkali whose causticity is blunted by a very feeble acid, and their similarity in this respect to the oleostearates, suggests their employment in the place of soap. Waterglass has indeed great cleansing power, and, as far as mere chemical effect is concerned, it should in most cases answer the purpose of soap. But the peculiar value of soap depends in no small degree on its mechanical action. The emulsive, as well as the solvent power, comes into play, and in many instances that soap is found most suitable that froths most. Waterglass shows little tendency to form an intimate mixture with oily substances, nor has it any disposition to foam. Soap leaves washed articles soft, while the silicate imparts an unpleasant harshness. Hence silicate of soda is objectionable for scouring wool and woolen or mixed fabrics. It is indeed better for washing or scouring, than a caustic alkali, since it does not have the same peculiar shrivelling effect on animal or vegetable substances. But it possesses little advantage over neutral carbonate of soda, in any respect. For cleaning paint however the silicate is said to be better than any thing else. Waterglass has been extensively tried against soap in large bleaching, dyeing, and printing establishments and in laundries, but though some say it does well enough, the reports are generally unfavorable.*

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* Wagner's Jahresbericht, iv, 172.

Patents have been obtained for soaps containing an admixture of silicate of soda, but until we have some proof that there is really anything to be gained by using such soaps, they are entitled to only a passing notice. I have made experiments to determine whether caustic soda could be economically made from the silicate, and the answer was decidedly in the negative.

5. Silicate of soda, on account of its chemical relations, has come into general use as a substitute for phosphate or arseniate of soda in dunging printed calicoes. This, the most important of all the applications, was patented in England by Jäger in 1852. The silicate by itself, however, was found to be too alkaline, and liable to dissolve away the aluminous mordants. Probably the article tried by many was not silicious enough, for if used alone, it ought to contain at least two equivalents of silica to one of soda. But such a silicate is very hard to dissolve, and what is prepared by chemical manufacturers, is commonly a sesquisilicate. Indeed Jäger himself prescribes such proportions as would form a sesquisilicate. In 1854 Higgin introduced a great improvement by substituting for waterglass the highly voluminous lime silicate formed by adding to silicate of soda, in the dunging vat, a sufficient quantity of chlorid of calcium to effect a complete double decomposition. This plan renders it perfectly safe to take the easily dissolved sesquisilicate of soda; and now waterglass has almost entirely superseded other dunging materials, being cheaper than any other substance, and in most cases giving perfect satisfaction. It is possible that in those few instances in which it has been rejected, due attention to the quality of the articles employed, would remove all difficulties. The fact that the alkaline silicates dissolve the protoxyd of iron, has been overlooked, and the manufacturer does not always take pains to peroxydize all the iron in the fused mass.

I have known an instance in which a lime salt was used, that happened to contain tarry matter and iron, and some pieces of cloth dunged with the mixture came out of the dyeing vat with the colors essentially degraded from the desired tints. It is reasonable to suppose then that a slight saddening of the bright colors, may result from taking a silicate that is brown, and allows dissolved protoxyd or sulphid of iron to go into the dunging mixture. And then again commercial muriatic acid often contains a reducing agent, sulphurous acid, and as some neutralize with chalk, which is always ferruginous, protochlorid of iron may be left in the lime solution. The chlorid of calcium should be made with milk of lime added in excess so as to insure the removal of every trace of iron. Pure salts of zinc, magnesia, or even of ammonia, would perhaps act as well as chlorid of calcium, but in most cases this is the cheapest substance to be had.

From peroxyd of iron, which has much less affinity for other bodies than the protoxyd, we have little to fear. In fact were the dunging bath free from the sesquioxyd to start with, it could not remain so, as it is continually receiving from the cloth the excess of the iron mordants.

According to Grüne,* silicate of soda may be made serviceable in dyeing cotton, since with its use cheaper mordants may be substituted for the acetates of alumina, iron, and tin. He pads the stuffs in a weak solution of waterglass, then passes them through alum, copperas, or protochlorid of tin, and after rinsing, colors them with any suitable dye-stuff. But his idea that silica contributes to the fastness of the colors, is hardly tenable. It is true that precipitated silica, under some circumstances, dries to a dense mass like sand, but when earths or oxyds are present, it more commonly takes the form of a very light, loose powder, and in this state, it can exert no protective power. If silica in fixing mordants forms a coating that resists acids and soaps, we might expect that after having once been dried, the cloth would be slow to receive the coloring matter itself. Yet printed calico dyes very well months after it has been dunged with waterglass. What little silica remains combined with mordants can be of little service further than to retain the oxyds in the active state.

There has lately appeared in the market, for the use of calico-printers, a substance professing to be more valuable than common waterglass, because it is a compound of silicate and arseniate of soda. A sample of it was found to contain, besides the silicate, 4·5 per cent of sulphate of soda, 3·4 per cent chlorid of sodium, and only 7·6 per cent of arseniate. But as sometimes more, sometimes less arsenic will be volatilized during the fusion of the ingredients, it would be difficult to make such a mixture of uniform composition. And then, too, seven or eight per cent of arseniate in the product, can have no special influence; or if it had, the printer might as well add a regular proportion of arseniate to the silicate solution, as pay an extra price for he knows not what.

Another absurd proposition which has been advanced by those wishing to monopolize the manufacture, is that a silicate containing an admixture of sulphate and chlorid, is superior to the pure article. There can be no reason assigned why it should be so, and as to experience, so far as I can learn, pure well worked waterglass has always given quite as good satisfaction in dunging calico, as either the arseniated silicate, or the uncertain, half made product prepared from impure soda ash. It has never yet been ascertained what is the precise reaction between the silicate of soda or lime and the mordant on the cloth. There can be little doubt that a silicate of alumina or iron is formed, and it is not improbable that this retains a small amount of lime or soda.

Manchester, N. H., August, 1861.

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* **Wagner's Jahresbericht, i, pp. 339, 340.**

ART. XXXIX.—*On the Fossil Fruits found in connection with the Lignites of Brandon, Vt.*; by LEO LESQUEREUX.

PROF. EDWARD HITCHCOCK (this Journal, 2d series, vol. xv, pag. 95–104,) has already given excellent descriptions accompanied with drawings, of the Brandon fruits and most satisfactory details concerning the strata with which they are connected. I owe to the celebrated Professor of Amherst College not only the communication of the original specimens from which the drawings have been made, but also a number of corresponding specimens that he had the kindness to present to me.

It cannot be expected that the examination I have been requested to make of these fruits can afford any exact botanical determinations. Indeed an accurate analysis of fossil plants is mostly impossible, their form being generally more or less obliterated and the preserved part, the hardest of course, being often of slight value, as a botanical character. By cutting a few specimens, I was enabled to find some details of anatomical structure in one of the species only, and thus to mark its botanical characters somewhat more accurately than it is generally done for fossil fruits. It is then only to point out the relation of some of the Brandon fruits with fossil species found elsewhere, or with genera of plants still living, and especially to try to come to a satisfactory understanding about the geological age of the lignite deposit where they are found, that the few following remarks are made:*

To facilitate further quotations and discussions about the Brandon fruits, I think that it is also convenient to give a short description of their essential characters and to name them.

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* The numbers marked in this paper correspond with those of the still unedited report of Vermont by Prof. Ed. Hitchcock. As they are not the same as some of the Journal, (loc. cit.) it is necessary to correlate them.

This Jour.,	Fig.	1	is equivalent to Fig.	111 to 117	of the Report.
“	“	2	“	118	“
“	“	3	“	119	“
“	“	4	“	132	“
“	“	5	“	133	“
“	“	6	“	123	“
“	“	7	“	145	“
“	“	8	“	142	“
“	“	9	“	146	“
“	“	10	“	134	“
“	“	11	“	136	“
“	“	12	“	126	“
“	“	13	“	153	“
“	“	14	“	154	“
“	“	15	“	151	“
“	“	16	“	148	“
“	“	17	“	149	“
“	“	18 and 19	“	150	“
“	“	20	“	159	

No. 1. *Carpolithes Brandoniana*, sp. nov. Capsule thick-walled, oval or nearly round, flattened, obtuse at both ends, valvate. Valves obscurely pointed, opening from the base to half the length of the capsule.

Var. α, elongata. Fig. 111 to 113.
Var. β, obtusa. Fig. 114 to 117.

Specimens figured 111 to 117 are certainly various forms of the same species and I think that those of 118, 119 and 124 ought to be referred to the same genus.

No. 2. *Carpolithes fissilis*, sp. nov. (Fig. 118, 119 and 124.) Capsule, a little flattened, ovate, lanceolate, obtuse or rounded at one end, pointed at the other, obscurely tencostate, irregularly tri-valved, dehiscent or closed. In this species as in the former, the size of the fruit varies from one to two inches in length and from half an inch to one inch or a little more in breadth.

Nothing like the fruits of these two species has been before published by palæontologists. The only fossil fruit to which they might be compared is the capsule of an *Embothrium* (Heer, Flor. tert. Helv., pl. 93, fig. 30,) and especially that of *Embothrium salignum*, a living species of the *Proteaceæ* family, a figure of which is given by the same author on the same plate. But the fruits of the genus *Embothrium* are borne on a strong woody stem generally preserved in the fossil state, while our fossil fruits of Brandon have no scars showing the point of attachment to a pedicel. They both, on the contrary, appear to have been enclosed, either in the spathe, or a fibrous capsule, like the fruits of some palms, or partly immersed in a cupula, like those of some *Cupuliferæ*. It may be that the thick, fibrous and woody capsule contained originally some seeds that have escaped by the dehiscence of the valves, as Prof. E. Hitchcock remarks; but the total absence of seeds, even within unopened specimens, led me to suppose that the thin pellicle which is seen within these peculiar fruits contained a mealy cotyledon, whose form and matter have been destroyed by maceration. It is useless however to speculate on these fruits till something more is known about their relation to some living species. Specimens of No. 1 are extremely abundant at Brandon.

No. 3. *Carpolithes irregularis*, sp. nov. (Fig. 120, 121, 123, 125, and 128.) Fruit capsular, about one inch in diameter, irregularly somewhat flattened, sometimes obscurely trigonal, round-oval, costate in its length.

These may be unopened and less flattened specimens of the same species as No. 2. They would compare well with some species of *Carya*, if they were not generally a little flattened or trigonal. Perhaps fig. 128 belongs to another species; but it looks like a deformed specimen of this.

No. 4. *Carpolithes Grayana*, sp. nov. (Fig. 122.) Fruit oval-elongated, obtuse at one end, marked by a sharp abrupt point at the other, a little flattened, one inch long, less than an inch broad, obscurely costate.

This species has just the form of the kernel of the almond. It is nearly related to *Carpolithes pruniformis* Heer, (l. c. vol. iii, p. 139, tab. 141, fig. 18 to 30,) abundant in the upper tertiary of Europe, especially at Œningen.

No. 5. *Carya verrucosa*, sp. nov. (Fig. 129.) Fruit oval, slightly costate, obtuse at both ends, warty.

This fruit is like *Carya Brauniana* Heer, (l. c., vol. iii, p. 93, tab. 127, fig. 50 and 51) from Œningen. It is a little larger than the following, but may be the same species still covered with the husk.

No. 6. *Carya Vermontana*, sp. nov. (Fig. 130.) Nut small, about half an inch long, oval, pointed at one end, obtuse at the other, six-costate.

It is extremely like *Carya Bruckmanni* Heer, (loc. cit., vol. iii, pag. 93, tab. 127, fig. 32) perhaps identical with it, also from Œningen, like the former.

No. 7. *Fagus Hitchcockii*, sp. nov. (Fig. 126 and 127.) Nut large, trigonal, with the angles somewhat obtuse, striated on the sides.

The fruit has nearly the same form as a nutlet of *Fagus ferruginea* Michx. It is only proportionally a little shorter and broader, and twice as large. Fig. 127 represents a specimen slightly open on one side at the point, with the angles more obtuse. It may be a different species or even belong to another genus. The fruit fig. 126 is indeed very large for the nut of a Beech; but Unger, in his Chloris Protogæa (pag. 101, tab. 27, fig. 1 to 4), has published *Fagus Deucalionis* with nuts as large as those of the Brandon species. It comes from the tertiary of Bohemia.

Genus *Apeïbopsis* (Heer). (*Cucumites* Bowerbank.) Prof. Heer has referred this genus to the family of *Tiliaceæ*, comparing it to *Apeïba*. It is characterized as follows: Fruit capsular, five to sixteen-valvate, polyspermous; seeds small, sub-globose, biseriate in each cell. On the characters of these fruits, the author further remarks: (Flor. tert. Helv., vol. iii, p. 38) "Where the bark of the fruit is preserved, it is marked with elongated warts and the fruit was externally verrucose. Within, it was probably filled with a fleshy matter containing the seeds in small cavities. In one specimen, (fig. 20 of Bowerbank) the seeds are placed without order in the central mass; in another (ibid., figs. 11, 12, 21, 34,) they appear to be placed in rows along the suture. Probably the fruit was divided into as many cells as there are furrows marked on the surface; but the walls were very thin and lost within the fleshy mass."

This description and the remarks of Prof. Heer agree well enough with what I was enabled to see by cutting a few of our

Brandon fruits, evidently referable to this genus, and by a microscopical examination of their internal structure. In the American species, the parietal follicle of the capsules appear thick and each one is distinct and separated from the other in its whole width, from the folding of it at the surface to the suture with the central axis, also pretty thick. There are thus internally six or seven loculi or carpels, separated by double walls. The seeds, very small indeed (less than one millimeter in diameter) and very numerous, fill the cells entirely and are apparently mixed with a fleshy or cellular matter. Those nearest to the placenta and of both sides of each cell appear placed in rows upon the whole width of the parietes, being there perpendicular to it, and more oval or a little longer than in the middle of the cells, where they are nearly globose. These seeds are enclosed in a yellowish somewhat pellucid envelope, easily separated from them and marked on its inner concave surface by regular black points (or papillæ?). The seeds appear also somewhat papillous or rugose, just like the outer surface of the fruit. The largest specimen of these fruits that I have seen is scarcely more than half an inch in diameter, nearly exactly globular, or a little elongated or oval. The surface is not regularly verrucose, but rather irregularly deeply rugose, marked by seven or eight furrows a little elevated on the borders, along the line of flexure of the parietal tissue. The extremities of the central axis are marked on both sides of the fruit by a small round scar, only a little larger at the point of attachment.

On the distribution of this remarkable genus, Prof. Heer says that it appears at first in the Eocene of England, and takes its greatest development in the lower Molasse of the Tertiary of Switzerland where four species have been found. It has also been found in the lower Miocene of Italy and also in Bohemia, and is then apparently lost. From this it would appear that as the Brandon deposit, where specimens of this genus are abundant, belongs apparently to the Upper Miocene, as will be seen presently, this genus has appeared later or persisted longer in America than in Europe. The size of our fruit, compared with that of the European Apeibopsis, (which have sometimes a diameter of two inches) shows that our species are diminutive. Although the general form of our specimens is somewhat alike, I think nevertheless that we have two species.

No. 8. *Apeibopsis Heerii*, sp. nov. (Fig. 131, 132, 133.) Fruit globular, deeply grooved or rugose, distinctly marked by seven furrowed costæ.

No. 9. *Apeibopsis Gaudini*, sp. nov. (Fig. 139 and 140.) Fruit smaller, oval, depressed on one side; costæ more numerous and less marked, surface nearly smooth.

It is on specimens of this last species that my examination was made. They appear abundant at Brandon but I have seen only three specimens of the former.

No. 10. *Aristolochia Œningensis*, Heer. (Fig. 134.) Fruit capsular, oval, six-costate, smooth or obscurely transversally rugose.

I cannot see any difference between our American specimen and Heer's figure of this species, except perhaps that the surface of the specimen appears somewhat transversally rugose. This appearance may be due to the process of maceration. Prof. Hitchcock's figure in this Journal (loc. cit. 1853) is quite the same as that of Heer (tab. 100, fig. 11, C. 1856), from a specimen of Œningen.

No. 11. *Aristolochia curvata*, sp. nov. (Fig. 135 and 136.) Fruit capsular, small, half an inch long, oval, pointed, marked with eight strong costæ, somewhat curved on one side.

No. 12. *Aristolochia obscura*, sp. nov. (Fig. 137, 138, and 141). Fruit capsular, small, one third of an inch in diameter, six or seven-costate, globular or a little flattened.

This species is uncertain. The specimens are not well preserved and I had not any for anatomical examination. I believe nevertheless that it is a specimen of this kind that Prof. Bailey has critically examined by a cross section. He found it a *six-valved pod, with seeds apparently flattened.* This agrees with the structure of the fruit of *Aristolochia.*

No. 13. *Sapindus Americanus*, sp. nov. (Fig. 142, 143, 144, 145.) Fruit oval-reniform, either smooth or irregularly rugose, depressed or flattened on one side, about half an inch in its greatest diameter.

These fruits have the general form of the fruit of *Cocculus Indicus* or of some fossil species of *Pavia.* They are smaller than the fruits of *Pavia* and moreover the cross section of one of the specimens (fig. 144) shows a fleshy cotyledonous substance enclosed in a thick putamen. The fruits also bear a marked round scar, showing a point of attachment at the upper end. It is not marked in the figure. These characters agree well enough with those of *Sapindus.* The nearest fossil species to that of No. 13 is *Sapindus lignitum* Ung., (l. c. pl. 1, page 33, tab. 6, fig. 3 to 5) from the lignites of Wetterau.

No. 14. *Carpolithes bursæformis,* sp. nov. (Fig. 146 and 147.) Fruit obovate, narrowed at one end, where it bears a round small cavity, inflated and obtuse at the other end, a little curved on one side, smooth.

I do not know of any fossil species to which this could be related. It is pear-shaped, as Prof. Hitchcock describes it; but it is a little curved on one side, a character that separates it from *Laurus.* As the specimens are much broken, this curved appearance may result from maceration, and if so, the species would well agree with *Laurus princeps* Heer, (l. c. vol. ii, page 77, tab. 89, fig. 176) from the upper tertiary of Europe, especially abundant at Œningen.

No. 15. *Cinnamomum Novæ-Angliæ*, sp. nov. (Fig. 148.) Fruit small, one sixth of an inch in diameter, globular, enlarged above, narrowed below to an obscurely costate point, apparently a broken pedicel, smooth.

I could not discover on the specimen the horizontal striæ marked on the figure. The fruit resembles that of a *Cinnamomum*, a genus well represented in the different stages of the tertiary of Europe. Of nine species described by Prof. Heer, five are found at Œningen. This genus as it is established by the same author is also represented in the Tertiary of America. Among a small number of fossil leaves collected by Dr. J. Evans in the Tertiary of Vancouver, there are specimens of two fine species of leaves of *Cinnamomum*. One other is abundant in the strata of the Tertiary of Mississippi (State).

No. 16. *Illicium lignitum*, sp. nov. (Fig. 149.) Seed small, one eighth of an inch long, oval, pointed, marked at the point by a small scar and by a ring on one side, very smooth and shining.

I can not but refer this seed to *Illicium*. It is a little thicker and more pointed than that of *Illicium anisatum* of China; but about the same size and the same form. Two species of *Illicium* are still living in the southern part of the United States (Gray's genera, vol. i, pag. 56.) On comparison with the published species of fossil plants, our seed is like the one figured by Göppert, under the general name of Drupa (Flora of Shossnitz, tab. 26, fig. 34). The tertiary of Shossnitz is of the same age as that of Œningen (Heer, vol. iii, pag. 306).

No. 17. *Drupa rhabdosperma*, sp. nov.? (Fig. 150.) Seed small, about of the same size and of the same form as the former, oval, pointed, or slightly beaked, finely and deeply striated, marked under the point by a deep triangular scar.

These seeds resemble those of *Pinus rhabdosperma* Heer, (vol. i, pag. 60, tab. 21, fig. 14) from the Miocene of Switzerland. The likeness is not enough to prove that our seeds are of the same species, or even of the same genus. Analogous forms of such small ribbed seeds are found in different genera. The putamen is pretty thick, very hard, bony, and in all the specimens that I have broken the kernel has been destroyed or the seed is empty. The kernel is covered with a brownish skin, like that enveloping the albumen of the seeds of the Pines. However the affinity of these seeds with those of a Pine is rendered doubtful by the absence of every trace of a wing in all the specimens, six in number, that I have seen.

No. 18. *Carpinus grandis?* Heer (l. c., vol. ii, pag. 40, tab. 72, fig. 15.) Nutlet oval, about one eighth of an inch in length, ribbed or striated.

I have not seen any specimen of this species. Prof. Hitchcock's figure 151 is like that of Prof. Heer for this species. *Carpinus Grandis* Heer, is from the upper Miocene of Switzerland.

No. 19. *Leguminosites pisiformis?* Heer (l. c., vol. iii, p. 129, tab. 133, fig. 37 to 40.) Seed globose, perfectly smooth, shining, one sixth of an inch broad.

Round small seeds like this are found in great number in the Tertiary of Europe and have been described under various names. The *Drupæ* (fig. 29 and 30 of tab. 26) of Göppert's flora of Shossnitz; the seeds of *Menianthes tertiaria* Heer (l. cit., vol. iii, pag. 20, tab. 104, fig. 20); some species of *Podogonium; Podogonium Knorrii* Heer (vol. iii, pag. 114, tab. 135), a characteristical plant of the upper Miocene of Europe, especially abundant at Œningen; and a number of *Carpolithes* or undetermined seeds. Though the identity of our Brandon *Leguminosites* with that of Œningen, (*L. pisiformis*) is far from certain, it is however remarkable that most of those fossil seeds that have about the same form as ours belong to the upper Tertiary.

No. 20. *Nyssa complanata*, sp. nov. (Fig. 153.) Fruit oval, a little flattened, bicostate with a deep furrow in the middle.

This species particularly resembles *Nyssa Vertumni* Ung., (Sill., pl. p. 16, tab. 8, fig. 19 and 20) from the lignites of the Rhine.

No. 21. *Nyssa microcarpa*, sp. nov. (Fig. 154.) Fruit oval, scarcely compressed, regularly ribbed, short.

Fig. 155 is longer; the point of the specimen is obliquely broken and thus its form can not be seen. It is perhaps some other species of the same genus. Our No. 21 is related to *Nyssa ornithobroma* Ung., (loc. cit., p. 16, tab. 8, fig. 15 and 18,) a species also from the lignites of Vetteravia.

No. 22. *Nyssa lævigata*, sp. nov. (Fig. 156.) Fruit cylindrical-oval, obtuse at one end, abruptly cut at the other, smooth.

The position of these fruits, as they are figured and their form, recall immediately the general appearance of the fruits of *Nyssa multiflora*. The likeness is still greater in comparing dry drupes of this species with the fossil specimens. The thick putamen of some *Nyssæ* is well adapted for preservation in the lignites. Though our fossil species is related to the living *Nyssa multiflora*, it differs by the size of the nutlets and the absence of striæ.

No. 23. *Carpolithes venosus?* Sternberg (Vers., vol. ii, pag. 208, tab. 58, fig. 18 to 20.) Fruit oval, about one inch long, irregularly and deeply sulcate and veined (fig. 157 to 160).

This is apparently a *Carya*. But the likeness with the species published by Sternberg, under the above name, is too great to permit a separation. Some pieces of the putamen, figured 159 and 160, have about the same thickness as that of *Carya olivæformis*, and are marked on the inner surface by irregularly crossed wrinkles like those of the *Carya*. This *Carpolithes venosus* is a species of the lignites of Bohemia.

To close this examination, I have still to mention a piece of the wood found in the lignites of Brandon. This wood, somewhat hardened and blackened, is still in a good state of preservation. It is soft enough to be cut with a knife, or at least easily broken, and on a section, it shows evidently the character of a Dicotyledonous wood. It cannot be specifically determined, of course; but it looks like the wood of a *Juglans* or a *Carya*.

Before I had had an opportunity of examining the fossil fruits of Brandon, and judging only from the drawings and descriptions published by Prof. Hitchcock (l. c.) I had, in a letter to Prof. J. D. Dana, given the opinion that the Brandon lignites were of the same age as the upper Tertiary deposits of Œningen. This opinion is fully confirmed by all that has been said above of the relation of those fruits with species of Œningen. It is true that the identity of species is not ascertained; but this, of course, can not be expected; and it is enough that the greatest number of the Brandon species are more generally related to species of Œningen, than to species of any other stage of the Tertiary, to authorize the above conclusion and to render it credible.

It is to be regretted that the fossil flora of the Tertiary of Mississippi is not better known. I have indeed found below Columbus, Ky., some specimens of fruits of a *Carpinus* and of a *Carya*, this last referable to *Carya olivæformis*, in the chalk banks, overlaid by ferruginous conglomerate. And in some red shales, received from Mississippi, I have found the fruit of a *Fagus*, resembling *Fagus ferruginea* if not identical with it. These data are too scanty to afford a point of comparison. But judging from the position of the lignites of Brandon, or rather from the nature of the strata overlying it, as it is described in Prof. Hitchcock's paper, I must believe that they are of the same age as the upper lignite formation that extends on both sides of the Mississippi, and which I had opportunity to explore in Arkansas, in company with my friend Prof. E. T. Cox. His general section of the Tertiary of Dallas county, Arkansas, presents in an exact manner a resumée of a number of local sections of that country. It is given in detail in the Geological Report of Arkansas by Dr. D. Dale Owen (vol. ii, p. 410) thus:—

General Section of the Tertiary of Dallas county, Arkansas.

Waterworn pebbles and gravel cemented by ferruginous conglomerate.

Place of fossil, generally silicified wood.

Red sandy clay, sometimes containing good iron ore and ferruginous sandstone, the last much fluted.

Light colored sand.

Upper lignite bed.
Ash-colored sandy clay.
Plastic potter clay.
Lower lignite.

The Copperas bluffs in St. Francis county, Ark., (same report, p. 418) show a section much like the above. A number of sections of the Geological report of Mississippi, by Dr. Eug. W. Hilgard, especially that of page 118, may be compared also with those of the strata accompanying the lignite deposit of Vermont. All show the same characters, viz., lignitic strata overlaid at some distance by strata of iron-ore or deposits of various kinds charged with iron. I know that it is still a question if all these lignite strata of the Mississippi shores, which I consider as upper tertiary, belong to the same age. Palæontology only can decide, when sufficient materials are collected. I will only remark that the lignites of Lauderdale, Miss., presenting with their accompanying strata a section resembling the above, are placed by Dr. Hilgard near the base of the tertiary, while their fossil plants show the greatest affinity with species of our time and are apparently of as recent an epoch as the fruits of Brandon, Vt.

Columbus, Ohio, Sept. 12, 1861.

ART. XL.—*Thirty-First Congress of the British Association for the Advancement of Science—with extracts from the Address of Mr. Fairbairn at the opening.*

THIS Congress assembled at Manchester on the 5th of September, and appears to have exceeded all others before held in the numbers present—in the amount of general and local subscriptions, (upon which the efficiency of the Association in promoting investigations mainly depends) in the value and number of the papers read, in the interest of the personal discussion, and in the excellence and variety of the evening discourses. Among the lectures we remark as of special interest the Astronomer Royal's (Airy) discourse on the eclipse of the sun; and Prof. Miller's lecture on the Spectrum Analysis.

The two subjects which commanded most general attention among those brought forward, were the Origin of Man, and Iron Plated Ships. The observations of Prof. Owen we shall publish as soon as they are received—those of Mr. Fairbairn we quote from his address.

The next meeting of the Association is to be held at Cambridge, when etiquette will probably require His Royal Highness the Prince of Wales to take the chair, who with such Vice Presidents as Dr. Whewell, Prof. Airy and Prof. Sedgwick will doubtless do honor to the Royal prerogative.

The sums recommended for special researches the coming year amounted to 2,363*l.* against 1,395*l.* last year. An Index to the Reports is in course of preparation, for which we observe an appropriation of 600*l.*

We copy from the London Athenæum the following extracts from:—

The Address of the President.

" *Gentlemen of the British Association*,—Ever since my election to the high office I now occupy I have been deeply sensible of my own unfitness for a post of so much distinction and responsibility; and when I call to mind the illustrious men who have preceded me in this chair, and see around me so many persons much better qualified for the office than myself, I feel the novelty of my position and unfeigned embarrassment in addressing you. I should however, very imperfectly discharge the duties which devolve upon me, as the successor of the distinguished nobleman who presided over the meetings of last year, if I neglected to thank you for the honorable position in which you have placed me, and to express at the outset my gratitude to those valued friends with whom I have been united for many years in the labors of the Sections of this Association, and from whom I have invariably received every mark of esteem."

"A careful perusal of the history of this Association will demonstrate that it was the first, and for a long time the only institution which brought together for a common object the learned Professors of our Universities and the workers in practical science. These periodical *réunions* have been of incalculable benefit in giving to practice that soundness of principle and certainty of progressive improvement which can only be obtained by the accurate study of science and its application to the arts. On the other hand, the men of actual practice have reciprocated the benefits thus received from theory, in testing by actual experiment deductions which were doubtful and rectifying those which were erroneous. Guided by an extended experience, and exercising a sound and disciplined judgment, they have often corrected theories apparently accurate, but nevertheless founded on incomplete data or on false assumptions inadvertently introduced. If the British Association had effected nothing more than the removal of anomalous separation of theory and practice, it would have gained imperishable renown in the benefit thus conferred. Were I to enlarge on the relation of the achievements of science to the comforts and enjoyments of man, I should have to refer to the present epoch as one of the most important in the history of the world. At no former period did science contribute so much to the uses of life and the wants of society. And in doing this it has only been fulfilling that mission which Bacon, the great father of modern science, appointed for it, when he wrote that "the legimate goal of the sciences is the endowment of human life with new inventions and riches," and when he sought for a natural philosophy which, not spending its energy on barren disquisitions, "should be operative for the benefit and endowment of mankind." Looking, then, to the fact that, while in our time all the sciences have yielded this fruit, engineering science, with which I have been most intimately connected, has preëmminently advanced the power, the wealth, and the comforts of mankind, I shall probably best discharge the duties of the office I have the honor to fill by stating, as briefly as possible, the more recent scientific discoveries which have so influenced the relations of social life. I shall, therefore, not dwell so much on the progress of abstract science, important as that is, but shall rather endeavor briefly to examine the applications of science to the useful arts, and the results which have followed, and are likely to follow, in the improvement of the condition of society." * * *

"In attempting to notice those branches of science with which I am but imperfectly acquainted, I shall have to claim your indulgence. This Association, as you are aware, does not confine its discussions and investigations to any particular science; and one great advantage of this is, that it leads to the division of labor, while the attention each department receives, and the harmony with which the plan has hitherto worked, afford the best guarantee of its wisdom and proof of its success." * * *

* * "Our knowledge of the physical constitution of the central body of our system seems likely, at the present time, to be much increased. The spots on the sun's disc were noticed by Galileo and his contemporaries, and enabled them to ascertain the time of its rotation and the inclination of its axis. They also correctly inferred, from their appearance, the existence of a luminous envelope, in which funnel-shaped depressions revealed a solid and dark nucleus. Just a century ago, Alexander Wilson indicated the presence of a second and less luminous envelope beneath the outer stratum; and his discovery was confirmed by Sir William Herschel, who was led to assume the presence of a double stratum of clouds, the upper intensely luminous, the lower grey, and forming the penumbra of the spots. Observations during eclipses have rendered probable the supposition that a third and outermost stratum of imperfect transparency incloses concentrically the other envelopes. Still more recently, the remarkable discoveries of Kirchoff and Bunsen require us to believe that a solid or liquid photosphere is seen through an atmosphere containing iron, sodium, lithium, and other metals in a vaporous condition. We must still wait for the application of more perfect instruments, and especially for the careful registering of the appearances of the sun by the photoheliograph of Sir John Herschel, so ably employed by Mr. Warren De La Rue, Mr. Welsh, and others, before we can expect a solution of all the problems thus suggested.

"Guided by the same principles which have been so successful in astronomy, its sister science, Magnetism, emerging from its infancy, has of late advanced rapidly in that stage of development which is marked by assiduous and systematic observation of the phenomena, by careful analysis and presentation of the facts which they disclose, and by the grouping of these in generalizations, which, when the basis on which they rest shall be more extended, will prepare the way for the conception of a general physical theory, in which all the phenomena shall be comprehended, while each shall receive its separate and satisfactory explanation. It is unnecessary to remind you of the deep interest which the British Association has at all times taken in the advancement of this branch of natural knowledge, or of the specific recommendations which, made in conjunction with the Royal Society, have been productive of such various and important results. To refer but to a single instance: we have seen those magnetic disturbances, so mysterions in their origin and so extensive in simultaneous prevalence,—and which, less than twenty years ago, were designated by a term specially denoting that their laws were wholly unknown,—traced to laws of periodical recurrence, revealing, without a doubt, their origin in the central body of our system, by inequalities which have for their respective periods the solar day, the solar year, and, still more remarkably, and until lately unsuspected, solar cycle of about ten of our terrestrial years, to whose existence they bear testimony in conjunction with the solar spots; but whose nature and causes are in all other respects still wrapped in entire obscurity. We owe to General Sabine, especially, the recognition and study of these and other solar magnetic influences, and of the magnetic influence of the moon similarly attested by concurrent determinations in many parts of the globe, which are now held to constitute a distinct branch of this science, not inappropriately named "celestial," as distinguished from purely terrestrial magnetism."

"We ought not in this town to forget that the very rapid advance which has been made in our time in Chemistry is due to the law of equivalents, or Atomic Theory, first discovered by our townsman, John Dalton. Since the development of this law, its progress has been unimpeded, and it has had a most direct bearing on the comforts and enjoyments of life." * * *

"The largest developments of chemistry, however, have been in connexion with the useful arts. What would now be the condition of calico-printing, bleaching, dyeing, and even agriculture itself, if they had been deprived of the aid of theoretic chemistry? For example, aniline—first discovered in coal tar by Dr. Hoffman, who has so admirably developed its properties—is now most extensively used as the basis of red, blue, violet, and green dyes. This important discovery will probably in a few years render this country independent of the world for dye stuffs; and it is more than probable that England, instead of drawing her dye stuffs from foreign countries, may herself become the centre from which all the world will be supplied. It is an interesting fact that at the same time, in another branch of this science, M. Tournet has lately demonstrated that the colors of gems, such as the emerald, aqua-marina, amethyst, smoked rock-crystal, and others, are due to volatile hydro-carbons, first noticed by Sir David Brewster in clouded topaz, and that they are not derived from metallic oxyds, as has been hitherto believed. Another remarkable advance has recently been made by Bunsen and Kirchoff in the application of the colored rays of the prism to analytical research. We may consider their discoveries as the commencement of a new era in analytical chemistry, from the extraordinary facilities they afford in the qualitative detection of the minutest traces of elementary bodies. The value of this method has been proved by the discovery of the new metals, *cæsium* and *rubidium*, by M. Bunsen; and it has yielded another remarkable result in demonstrating the existence of iron and six other known metals in the sun. In noticing the more recent discoveries in this important science I must not pass over in silence the valuable light which chemistry has thrown upon the composition of iron and steel. Although Despretz demonstrated many years ago that iron would combine with nitrogen, yet it was not until 1857 that Mr. C. Binks proved that nitrogen is an essential element of steel; and more recently M. Caron and M. Frémy have further elucidated this subject; the former showing that cyanogen, or cyanid of ammonium, is the essential element which converts wrought iron into steel; the latter combining iron with nitrogen through the medium of ammonia, and then converting it into steel by bringing it at the proper temperature into contact with common coal gas. There is little doubt that in a few years these discoveries will enable Sheffield manufacturers to replace their present uncertain, cumbrous, and expensive process by a method at once simple and inexpensive, and so completely under control as to admit of any required degree of conversion being obtained with absolute certainty. Mr. Crace Calvert, also, has proved that cast iron contains nitrogen, and has shown that it is a definite compound of carbon and iron, mixed with various proportions of metallic iron, according to its nature. Before leaving chemical science, I must refer to the interesting discovery by M. Deville, by which he succeeded in rapidly melting thirty-eight or forty pounds of platinum—a metal till then considered almost infusible. This discovery will render the extraction of platinum from the ore more perfect, and, by reducing its cost, will greatly facilitate its application to the arts."

"It is litttle more than half a century since geology assumed the distinctive character of a science. Taking into consideration the aspects of nature in different epochs of the history of the earth, it has been found that the study of the changes at present going on in the world around us enables us to understand the past revolutions of the globe, and the conditions and circumstances under which strata have been formed and organic remains embedded and preserved. The geologist has increasingly tended to believe that the changes which have taken place on the face of the globe, from the earliest times to the present, are the result of agencies still at work. But while it is his high office to record the distribution of life in past ages, and the evidence of physical changes in the arrangement of land and water, his results hitherto have indicated no traces of its beginning, nor have they afforded evidence of the time of its future duration. Geology has been indebted for this progress very largely to the investigations of Sedgwick and the writings of Sir Charles Lyell." * *

"It is well known that the temperature increases as we descend through the earth's crust, from a certain point near the surface, at which the temperature is constant. In various mines, borings, and artesian wells the temperature has been found to increase about one degree Fahrenheit for every sixty or sixty-five feet of descent. In some carefully conducted experiments during the sinking of Dukinfield Deep Mine—one of the deepest pits in this country—it was found that a mean increase of about one degree in seventy-one feet occurred. If we take the ratio thus indicated, and assume it to extend to much greater depths, we should reach at two and a half miles from the surface strata at the temperature of boiling water; and at depths of about fifty or sixty miles the temperature would be sufficient to melt, under the ordinary pressure of the atmosphere, the hardest rocks. Reasoning from these facts, it would appear that the mass of the globe, at no great depth, must be in a fluid state. But this deduction requires to be modified by other considerations, namely, the influence of pressure on the fusing point, and the relative conductivity of the rocks which form the earth's crust. To solve these questions a series of important experiments were instituted by Mr. Hopkins, in the prosecution of which Dr. Joule and myself took part; and after a long and laborious investigation it was found that the temperature of fluidity increased about one degree Fahrenheit for every five hundred pounds of pressure in the case of spermaceti, beeswax, and other similar substances. However, on extending these experiments to less compressible substances, such as tin and barytes, a similar increase was not observed. But this series of experiments has been unavoidably interrupted; nor is the series on the conductivity of rocks entirely finished. Until they have been completed by Mr. Hopkins, we can only make a partial use of them in forming an opinion of the thickness of the earth's solid crust. Judging, however, alone from the greater conductivity of the igneous rocks, we may calculate that the thickness cannot possibly be less than nearly three times as great as that calculated on the usual suppositions of the conductive power of the terrestrial mass at enormous depths being no greater than that of the superficial sedimentary beds. Other modes of investigation which Mr. Hopkins has brought to bear on this question appear to lead to the conclusion that the thickness of the earth's crust is much greater even than that above stated. This would require us to assume that a part of the heat in the crust is due to superficial and external rather than central causes. This does not bear directly against the doctrine of central heat, but shows that only a part of the increase of temperature observed in mines and deep wells is due to the outward flow of that heat." * * *

"Two other branches of scientific research, Geography and Ethnology, have for some years been united, in this Association, in one Section, and that probably the most attractive and popular of them all. We are much indebted to Sir Roderick Murchison, among other members of the Association, for its continued prosperity, and the high position it has attained in public estimation. The spirit of enterprise, courage and perseverance displayed by our travellers in all parts of the world have been powerfully stimulated and well supported by the Royal Geographical Society; and the prominence and rapid publicity given to discoveries by that body have largely promoted geographical research. In Physical Geography, the late Baron von Humboldt has been one of the largest contributors, and we are chiefly indebted to his personal researches and numerous writings for the elevated position it now holds among the sciences. To Humboldt we owe our knowledge of the physical features of Central and Southern America. To Parry, Sir James Ross and Scoresby we are indebted for discoveries in the Arctic and Antarctic regions. Geography has also been advanced by the first voyage of Franklin down the Copper Mine River, and along the inhospitable shores of the Northern seas, as far as Point Turnagain; as also by that ill-fated expedition in search of a northwest passage, followed by others in search of the unfortunate men who perished in their attempt to reach those ice-bound regions, so often stimulated by the untiring energy of a high-minded woman. In addition to these, the discoveries of Dr. Livingstone in Africa have opened to us a wide field of future enterprise along the banks of the Zambesi and its tributaries. To these, we may add the explorations of Capt. Burton in the same continent, and those also by Capt. Speke and Capt. Grant of a hitherto unknown region, in which it has been suggested that the White Nile has its source, flowing from one of two immense lakes, upwards of 300 miles long by 100 broad, and situated at an elevation of 4,000 feet above the sea. To these remarkable discoveries I ought to add an honorable mention of the sagacious and perilous exploration of Central and Northern Australia by Mr. M'Douall Stuart.

Having glanced, however imperfectly, at some of the most important branches of science which engage the attention of Members of this Association, I would now invite attention to the Mechanical Sciences, with which I am more familiarly acquainted. They may be divided into Theoretical Mechanics and Dynamics, comprising the conditions of equilibrium and the laws of motion; and Applied Mechanics, relating to the construction of machines. I have already observed that Practice and Theory are twin sisters, and must work together to insure a steady progress in mechanical art. Let us, then, maintain this union as the best and safest basis of national progress, and, moreover, let us recognize it as one of the distinctive aims of the annual *réunions* of this Association. During the last century, the science of Applied Mechanics has made strides which astonish by their magnitude; but even these, it may reasonably be hoped, are but the promise of future and more wonderful enlargements. I therefore propose to offer a succinct history of these improvements, as an instance of the influence of scientific progress on the well-being of society. I shall take in review the three chief aids which engineering science has afforded to national progress,—namely, canals, steam navigation and railways; each of which has promoted an incalculable extension of the industrial resources of the country. One hundred years ago, the only means for the conveyance of inland merchandise were the packhorses and wagons on the then imperfect highways. It was reserved for Brindley, Smeaton and others to introduce a system of canals, which opened up facilities for an interchange of commodities at a cheap rate over almost every part of the country. The impetus given to industrial operations by this new system of conveyance induced capitalists to embark in trade, in mining, and in the extension of manufactures in almost every district. These improvements continued for a series of years, until the whole country was intersected by canals, requisite to meet the demands of a greatly extended industry. But canals, however well adapted for the transport of minerals and merchandise, were less suited for the conveyance of passengers. The speed of the canal boats seldom exceed from 2½ to 3 miles an hour; and, in addition to this, the projectors of canals sometimes sought to take an unfair advantage of the Act of Parliament, which fixed the tariff at so much per ton per mile, by adopting circuitous routes, under the erroneous impression that mileage was a consideration of great importance in the success of such undertakings. It is in consequence of short-sighted views and imperfect legislation that we inherit the numerous curves and distortions of our canal system." * *

"Scarcely had the canal system been fully developed when a new means of propulsion was adopted—namely, steam. I need not recount to you the enterprise, skill and labor that have been exerted in connection with steam navigation. You have seen its results on every river and sea; results we owe to the fruitful minds of Miller, Symington, Fulton, and Henry Bell, who were the pioneers in the great march of progress. Viewing the past, with a knowledge of the present and a prospect of the future, it is difficult to estimate sufficiently the benefits that have been conferred by this application of mechanical science to the purposes of navigation. Power, speed, and certainty of action have been attained on the most gigantic scale. The celerity with which a modern steamer, with a thousand tons of merchandise and some hundreds of human beings on board, cleaves the water and pursues her course far surpasses the most sanguine expectations of a quarter of a century ago, and indeed almost rivals the speed of the locomotive itself. Previous to 1812 our intercourse with foreign countries and with our colonial possessions depended entirely upon the state of the weather. It was only in favorable seasons that a passage was open, and we had often to wait days, or even a week, before Dublin could be reached from Holyhead. Now this distance of sixty-three miles is accomplished in all weathers in little more than three hours. The passage to America used to occupy six weeks, or two months; now it is accomplished in eight or nine days. The passage round the Cape to India is reduced from nearly half a year to less than a third of that time, while that country may be reached by the overland route in less than a month.

"These are a few of the benefits derived from steam navigation, and, as it is yet far from perfect, we may reasonably calculate on still greater advantages in our intercourse with distant nations. I will not here enter on the subject of the numerous improvements which have so rapidly advanced the progress of this important service. Suffice it to observe that the paddle-wheel system of propulsion has maintained its superiority over every other method yet adopted for the attainment of speed, as by it the best results are obtained with the least expenditure of power. In ships of war the screw is indispensable, on account of the security it affords to the engines and machinery, from their position in the hold below the water line, and because of the facility it offers in the use of sails, when the screw is raised from its position in the well to a recess in the stern prepared for that purpose. It is also preferable in ships which require auxiliary power in calms and adverse winds, so as to expedite the voyage and affect a considerable saving upon the freight.

"The public mind had scarcely recovered itself from the changes which steam navigation had caused, and the impulse it had given to commerce, when a new and more gigantic power of locomotion was inaugurated. Less than a quarter of a century had elapsed since the first steamboats floated on the waters of the Hudson and the Clyde, when the achievements thence resulting were followed by the application of the same agency to the almost superhuman flight of the locomotive and its attendant train. I well remember the competition at Rainhill in 1830, and the incredulity everywhere evinced at the proposal to run locomotives at 20 miles an hour. Neither George Stephenson himself, nor any one else had at that time the most distant idea of the capabilities of the railway system. On the contrary, it was generally considered impossible to exceed 10 or 12 miles an hour; and our present high velocities, due to high-pressure steam and the tubular system of boilers, have surpassed the most sanguine expectations of engineers. The sagacity of George Stephenson at once seized upon the suggestions of Henry Booth, to employ tubular boilers; and, that, united to the blast-pipe, previously known, has been the means of affecting all the wonders we now witness in a system that has done more for the development of practical science and the civilization of man than any discovery since the days of Adam." * * *

Want of space compels us to pass Mr. Fairbairn's very interesting remarks on the history of the steam engine and the manufacturing industry of Great Britain.

"I might greatly extend this description of our manufacturing industry, but I must for the present be brief, in order to point out the dependence of all these improvements on the iron and coal so widely distributed among the mineral treasures of our island. We are highly favored in the abundance of these minerals, deposited with an unsparing hand by the great Author of Nature, under so slight a covering as to bring them within reach of the miner's art. To them we owe our present high state of perfection in the useful arts; and to their extended application we may safely attribute our national progress and wealth. So that, looking to the many blessings which we daily and hourly receive from these sources alone, we are impressed with devotional feelings of gratitude to the Almighty for the manifold bounties He has bestowed on us. Previously to the inventions of Henry Cort, the manufacture of wrought iron was of the most crude and primitive description. A hearth and a pair of bellows was all that was employed. But since the introduction of puddling, the iron-masters have increased the production to an extraordinary extent, down to the present time, when processes for the direct conversion of wrought iron on a large scale are being attempted. A consecutive series of chemical researches into the different processes, from the calcining of the ore, to the production of the bar, carried on by Dr. Percy and others, has led to a revolution in the manuafcture of iron; and although it is at the present moment in a state of transition, it nevertheless requires no very great discernment to perceive that steel and iron of any required tenacity will be made in the same furnace, with a facility and certainty never before attained. This has been effected to some extent by improvements in puddling; but the process of Mr. Bessemer—first made known at the meeting of this Association in Cheltenham—affords the highest promise of certainty and perfection in the operation of converting the melted pig direct into steel or iron, and is likely to lead to the most important developments in this manufacture. These improvements in the production of the material must, in their turn, stimulate its application on a larger scale and lead to new constructions.

In iron shipbuilding an immense field is opening before us. Our wooden walls have to all appearances seen their last days; and as one of the early pioneers in iron construction, as applied to shipbuilding, I am highly gratified to witness a change of opinion that augurs well for the securities of the liberties of the country. From the commencement of iron shipbuilding in 1830 to the present time, there could be only one opinion among those best acquainted with the subject, namely, that iron must eventually supersede timber in every form of naval construction. The large ocean steamers, the Himalaya, the Persia, and the Great Eastern, abundantly show what can be done with iron, and we have only to look at the new system of casing ships with armor plates, to be convinced that we can no longer build wooden vessels of war with saftey to our naval superiority and the best interests of the country. I give no opinion as to the details of the reconstruction of the navy—that is reserved for another place—but I may state that I am fully persuaded that the whole of our ships of war must be rebuilt of iron, and defended with iron armor calculated to resist projectiles of the heaviest description at high velocities. In the early stages of iron shipbuilding, I believe I was the first to show, by a long series of experiments, the superiority of wrought iron over every other description of material in security and strength, when judicially applied in the construction of ships of every class. Other considerations, however, affect the question of vessels of war; and although numerous experiments were made, yet none of the targets were on a scale sufficient to resist more than a 6-pounder shot. It was reserved for our scientific neighbors, the French, to introduce thick iron plates as a defensive armor for ships. The success which has attended the adoption of this new system of defence affords the prospect of invulnerable ships of war, and hence the desire of the government to remodel the Navy on an entirely new principle of construction, in order that we may retain its superiority as the great bulwark of the nation. A committee has been appointed by the war-office and the admiralty for the purpose of carrying out a scientific investigation of the subject, so as to determine, first, the best description of material to resist projectiles; secondly, the best method of fastening and applying that material to the sides of ships and land fortifications; and, lastly, the thickness necessary to resist the different descriptions of ordnance.

"It is asserted, probably with truth, that whatever thickness of plates is adopted for casing ships, guns will be constructed capable of destroying them. But their destruction will even then be a work of time, and I believe, from what I have seen in recent experiments, that with proper armor it would require, not only the most powerful ordnance, but also a great concentration of fire, before fracture will ensue. If this be the case, a well-constructed iron ship, covered with sound plates of the proper thickness, firmly attached to its sides, will, for a considerable time, resist the heaviest guns which can be brought to bear against it, and be practically shot-proof. But our present means are inadequate for the production of large masses of iron, and we may trust that, with new tools and machinery, and the skill, energy, and perseverance of our manufacturers, every difficulty will be overcome, and armor plates produced which will resist the heaviest existing ordnance. The rifling of heavy ordnance, the introduction of wrought iron, and the new principle of construction with strained hoops, have given to all countries the means of increasing enormously the destructive power of their ordnance. One of the results of this introduction of wrought iron and correct principles of manufacture, is the reduction of the weight of the new guns to about two-thirds the weight of the older cast-iron ordnance. Hence follows the facility with which guns of much greater power can be worked, while the range and precision are at the same time increased. But these improvements cannot be confined to ourselves. Other nations are increasing the power and range of their artillery in a similar degree, and the energies of the nation must, therefore, be directed to maintain the superiority of our navy in armor as well as in armament." * * *

"Among the changes which have large contributed to the comfort and enjoyment of life, are the improvements in the sanitary condition of towns. These belong probably to the province of social rather than mechanical science; but I cannot omit to notice some of the great works that have of late years been constructed for the supply of water and for the drainage of towns. In former days 10 gallons of water to each person per day was considered an ample allowance. Now, 30 gallons is much nearer the rate of consumption. I may instance the water-works of this city and of Liverpool, each of which yield a supply of from 20 to 30 gallons of water to each inhabitant. In the former case the water is collected from the Cheshire and Derbyshire hills, and after being conveyed in tunnels and aqueducts a distance of 10 miles to a reservoir, where it is strained and purified, it is ultimately taken a further distance of eight miles in pipes, in a perfectly pure state, ready for distribution. The greatest undertaking of this kind, however, yet accomplished is that by which the pure waters of Loch Katrine are distributed to the city of Glasgow. This work, recently completed by Mr. Bateman, who was also the constructor of the water works of this city, is of the most gigantic character, the water being conveyed in a covered tunnel a distance of 27 miles through an almost impassable country to the service reservoir, about eight miles from Glasgow. By this means 40 million gallons of water per day are conveyed through the hills which flank Ben Lomond, and after traversing the sides of Loch Chon and Loch Aird, are finally discharged into the Mudgdock Basin, where the water is impounded for distribution. We may reasonably look forward to an extension of similar benefits to the metropolis, by the same Engineer, whose energies are now directed to an examination of the pure fountain of Wales, from whence the future supply of water to the great city is likely to be derived. A work of so gigantic a character may be looked upon as problematical, but when it is known that six or seven millions of money would be sufficient for its execution, I can see no reason why an undertaking of so much consequence to the health of London should not ultimately be accomplished." * * *

"A brief allusion must be made to that marvellous discovery which has given the present generation the power to turn the spark of heaven to the uses of speech; to transmit along the slender wire for a thousand miles a current of electricity that renders intelligible words and thoughts. This wonderful discovery, so familiar to us, and so useful in our communications to every part of the globe, we owe to Wheatstone, Thompson, De la Rive, and others. In land telegraphy the chief difficulties have been surmounted, but in submarine telegraphy much remains to be accomplished. Failures have been repeated so often as to call for a commission on the part of Government to inquire into the causes, and the best means of overcoming the difficulties which present themselves. I had the honor to serve on that commission, and I believe that from the report, and mass of evidence and experimental research accumulated, the public will derive very important information. It is well known that three conditions are essential to success in the construction of ocean telegraphs—perfect insulation, external protection, and appropriate apparatus for laying the cable safely on its ocean bed. That we are far from having succeeded in fulfilling these conditions is evident from the fact that out of 12,000 miles of submarine cable which have been laid since 1851, only 3,000 miles are in actual working order; so that three-fourths may be considered as a failure and loss to the country. The insulators hitherto employed are subject to deterioration from mechanical violence, from chemical decomposition or decay, and from the absorption of water; but the last circumstance does not appear to influence seriously the durability of cables. Electrically, india-rubber possesses high advantages, and, next to it, Wray's compound and pure gutta percha far surpass the commercial gutta percha hitherto employed; but it remains to be seen whether the mechanical and commercial difficulties in the employment of these new materials can be successfully overcome. The external projecting covering is still a subject of anxious consideration. The objections to iron wire are its weight and liability to corrosion. Hemp has been substituted, but at present with no satisfactory result. All these difficulties, together with those connected with the coiling and paying out of the cable, will no doubt yield to careful experiment and the employment of proper instruments in its construction, and its final deposit on the bed of the ocean. Irrespective of inland and international telegraphy, a new system of communication has been introduced by Prof. Wheatstone, whereby intercourse can be carried on between private families, public offices, and the works of merchants and manufacturers. This application of electric currents cannot be too highly appreciated, from its great efficiency and comparatively small expense. To show to what an extent this improvement has been carried, I may state that 1,000 wires in a perfect state of insulation, may be formed into a rope not exceeding half an inch in diameter." * *

ART. XLI.—*Note on a Compound of Ammonia, Mercury and Nitric Acid;* by M. CAREY LEA, Philadelphia.

IN the course of an extended examination upon the reactions of the ethyl bases, I noticed that both ethylamine and diethylamine gave with acid solution of mercuric nitrate a white precipitate which is permanent although the solution contains a large excess of acid. This compound did not appear worthy of a special examination as it is no doubt analogous to that formed by ammonia under similar circumstances. But with respect to the constitution of this latter I propose to offer a few remarks.

The formula adopted for this compound by L. Gmelin, apparently on the authority of Kane, C. G. Mitscherlich and Pagenstecher, whose analyses are quoted, is

$$HgNH_2+2HgO, NO_5.$$

This view of the constitution is liable to two objections; first, that it supposes the existence of the bibasic nitrate of mercury in it, which substance is so unstable that it is decomposed by mere contact with cold water; second, that it is unnecessarily complicated. If we assign to the compound the formula

$$\left.\begin{matrix}Hg_3\\H\end{matrix}\right\}NO, NO_5+2HO,$$

differing from the foregoing by 1 at. H and 1 at. O, we greatly simplify our view of its constitution, and refer it to the class of substituted ammonias.

Adopting this view, we are able to explain its formation by the action of ammonia on mercuric nitrate, by a very natural equation

$$3(HgONO_5)+NH_3=\underbrace{\left.\begin{matrix}Hg_3\\H\end{matrix}\right\}NO, NO_5+2HO}_{\text{new body}}+2NO_5$$

(unless ammonia be added in excess, the solution always remains strongly acid).

Not only is the formula here proposed more in harmony with the views entertained at the present day, which tend as far as possible to refer such compounds to the ammonia type, but, what is somewhat remarkable, it accords with the analyses of all three of the chemists just mentioned, much better than the formula hitherto adopted. To make this apparent I subjoin them. In the second column opposite the word ammonia I have grouped together the atoms NH_3 derived partly from the substituted ammonia and partly from the 2HO in order to complete the comparison of the results of calculation and analysis.

	Calculated according to the old formula $HgNH_2+2HgONO_5$.	Calc. by the formula here proposed $\left.\begin{matrix}Hg_3\\H\end{matrix}\right\}NONO_5+2HO$.	Found.		
			Kane.	C. G. Mitscherlich.	Pagenstecher.
Mercury,	77·72	75·95	76·41	75·47	74·12
Ammonia, $\left\{\begin{matrix}Amid\\NH_2\end{matrix}\right\}$	4·15	(NH_3) 4·30	3·78	4·40	
Oxygen,	(2O) 4·15	(3O) 6·08		6·05	5·92
Nitric acid,	13·98	13·67	12·66	14·33	$\left.\begin{matrix}NH_4O\\NO_5\end{matrix}\right\}$ 17·40
	100·00	100·00		100·25	

It becomes at once evident on inspection that the formula I here propose is greatly more in accordance with the analyses than the old one. The mercury agrees better with all of them; the ammonia and nitric acid at least equally well. But it is (as was to be expected) in the oxygen that the difference is most striking. Mitscherlich and Pagenstecher found 6·05 and 5·92: the new formula gives 6·08, the old 4·15. This would seem to be conclusive.

Philadelphia, Oct. 1st, 1861.

ART. XLII.—*An Instrument designed to illustrate certain resultant vibrations in Polarized Light;* by Prof. E. S. SNELL, Amherst College.

I HAVE lately designed and constructed an instrument for exhibiting the combinations of two rays of light, polarized in planes at right angles to each other, when they coincide in direction, but one is in advance of the other by any fraction whatever of the wave-length.

I am aware that an instrument for presenting these resultant forms and motions has been invented by Plucker, a German physicist. But it appears to me that its mechanical difficulties must be so serious, as to render it practically useless. There is also much inconvenience in attempting to show by it one polarization by itself, then the other, and finally some resultant of both combined. Besides, after sending forward two or three waves at most, the process comes to an end, and there must be a backward movement, before another progression of waves can be shown. I have never seen one of Plucker's instruments in operation, but the description and engravings satisfy me, that unless the parts are almost absolutely free from friction, the rods will bind on the surfaces and in the grooves, and stop all progress, or become bent in the attempt to continue the motion.*

The apparatus here described is free from these several objections. The friction of the parts has no tendency to make them bind against each other, and thus check the motion of the waves; the change can be made in a moment from each single polarized system to the other, and to any desired combination of the two systems; and finally, the series of waves, of whatever kind, passes along uninterruptedly, so long as the experimenter chooses.

The general plan of giving motion to the rods which carry the balls is the same as that employed in the instrument for showing the molecular motion in water-waves, an account of which was published in this Journal, October, 1845, and which has recently been figured and described in Mr. Ritchie's catalogue of philosophical apparatus. Fig. 1 is a perspective view of the instrument, when opened so as to show the mechanism. The balls at the top represent so many particles of the luminiferous ether. They receive their vertical motions from the cams on the lower axis, which is turned by the crank (*a*); and their horizontal motions from a similar system of cams on the upper axis, which is made to revolve by a band from the lower. Fig. 2 presents a view of one of these systems of cams, twenty-four in number, of circular section, and arranged in a regular helix, making two revolutions from end to end. The upper and lower axes, so far as the cams are concerned, are precisely alike, and parallel; and by means of a metallic band and pins, they have the same velocity of revolution, whenever the lower one is turned. The lower axis is placed near the front of the box, and on each cam rests a horizontal rod or bar of wood, eighteen inches long, free to revolve in a vertical plane on a pivot at the back side of the box. The rods lie between guides, which prevent lateral motion. On the front extremity of each bar there stands another bar, upright, fifteen inches long, the two being pivoted together by a joint which allows free motion. These vertical rods also play between guides, (*c*), which permit motion in vertical planes coincident with those in which the horizontal bars respectively move. An elastic cord, about three inches long, is fastened to each pair of rods near their angle of meeting, and tends by its contraction to hold back the vertical rod against its cam on the upper axis. Inserted in the extremity of each vertical rod is a black iron wire, reaching through a slit in the top of the box, having a white ball upon it, which thus appears in sight, when all the mechanism is concealed from view.* The whole vertical rod, from the hinge to the ball, is eighteen inches long, the same as the horizontal bar. Fig. 1 presents one of the resultant forms of wave, when the rods are in contact with their respective cams on both axes.

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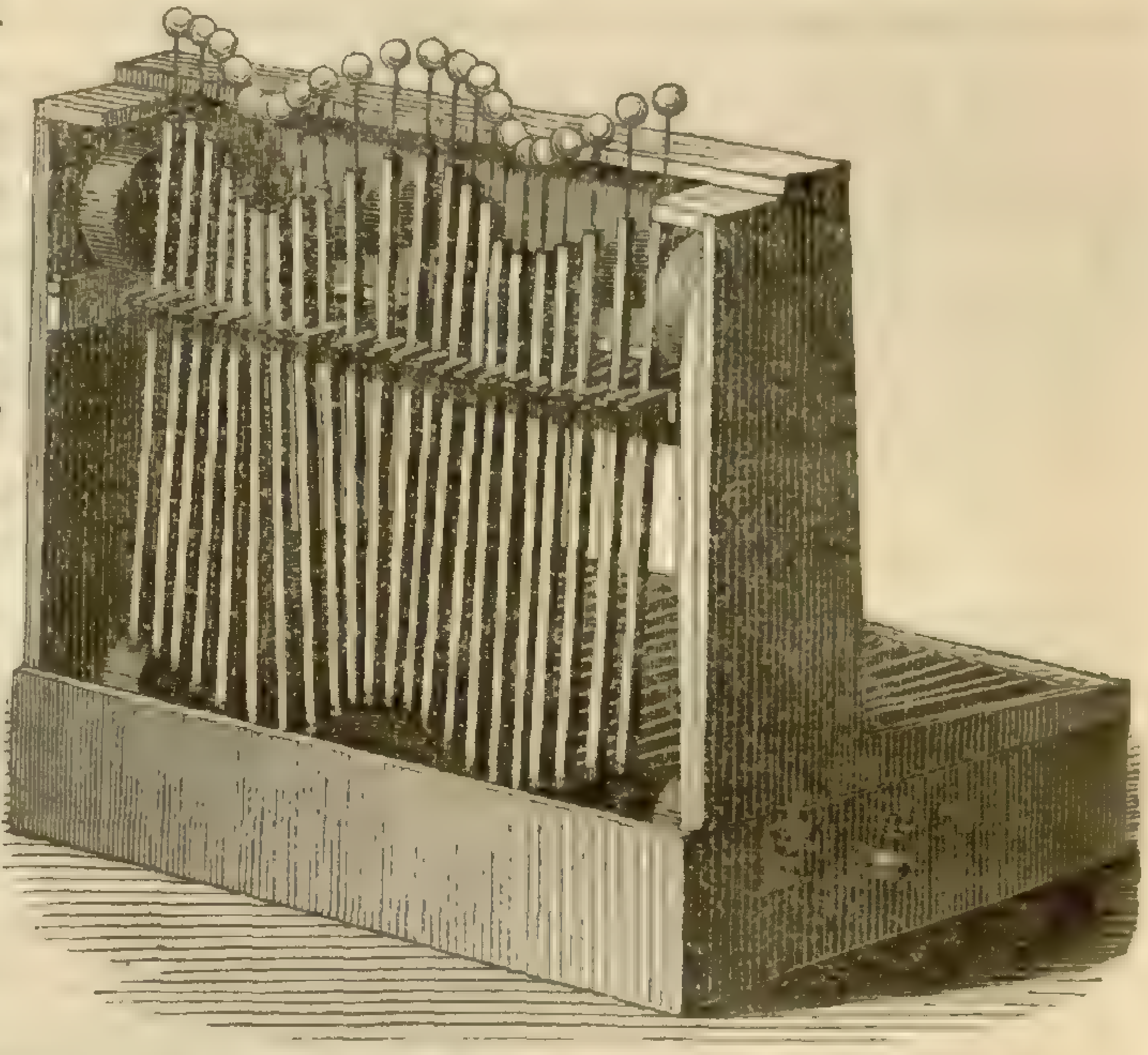
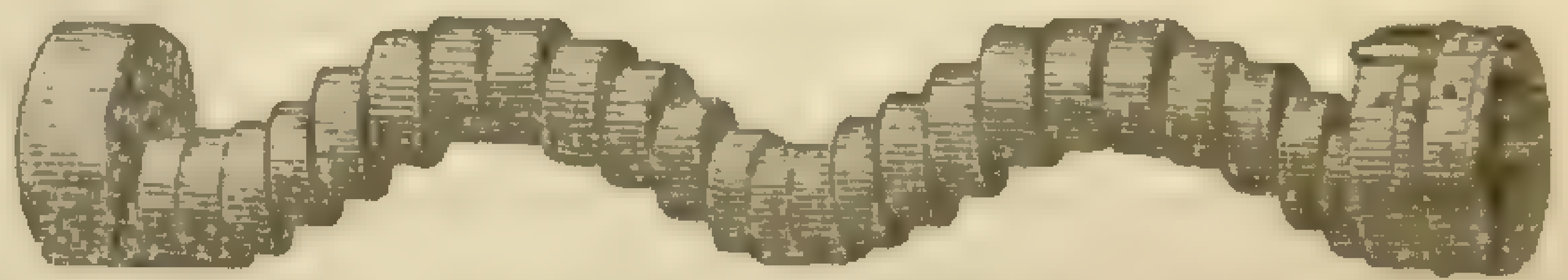
* For a description and representations of Plucker's instrument, see "Introduction a la Haute Optique, par A. Beer, traduit par M. C. Forthomme," pp. 148–153.

1.



2.



Back of the lower axis, and below the horizontal bars, lies a strip of board, which can be turned up on hinges, so as to lift all the bars off from the cams; a similar piece is hinged below the upper axis, and behind the vertical rods, which, being turned into a horizontal position, presses the rods forward, and releases them from the upper cams. While they are placed thus, all the lower bars lie in a horizontal plane, and the upright ones in a vertical plane, so that the balls are in a straight line, representing the particles of ether at rest. If the crank is turned, both systems of cams revolve, but produce no motion of the balls. If the lower support is turned down, the horizontal bars fall upon their respective cams, the balls assume the arrangement of vertical waves, and on turning the crank, the wave-motion is seen in a *vertical plane* alone, representing a *plane-polarized pencil of light.* Now let the lower support be raised again, and the upper one be turned down; then the vertical rods are held back by the elastic cords against the upper system of cams, while the pivots at the bottom are in a horizontal line. If the crank is now turned, the lower cams produce no effect, but the upper system, revolving by the band, gives the balls a wave-motion in a *horizontal plane*, thus representing a pencil of light *polarized in a plane at right angles to the former.* Finally, if the lower support is once more let down, and the crank turned, the vertical waves coexist with the horizontal ones, and *some resultant mode of polarization is represented*, the precise character of which depends on the relation of phases in the two systems. The contrivance for varying the species of polarization, by bringing together any phases we please, is very simple. On the lower axis, the cams are all firmly attached to the short cylinder about which the band passes; but on the upper axis, the cylinder is divided, and the band passes over the detached portion or wheel, which is free to revolve on the central axis, but which can be held in any give position upon it, by a tooth on the main cylinder, which tooth is forced by a spring into a notch on the edge of the wheel. The wheel and spring are seen in fig. 2. Hence, by raising the tooth and turning the crank, the lower axis revolves, giving rotation also to the wheel, but not to the upper axis, until the balls assume the required relation to each other, when the tooth is dropped into the corresponding notch, and the desired polarization is produced, and can be continued at pleasure.

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* The top piece containing the slits is not represented in the figure.

The resultant modes of polarization, as theory shows, are the following:

(1.) *Plane polarization*, the vibrations being in a plane inclined 45° to the right.

(2.) *The same*, the plane inclined 45° to the left.

(3.) *Circular polarization*, the atoms revolving from the right over to the left.

(4.) *The same*, the revolution being from left over to right.

(5.) *Elliptical polarization*, the transverse axis inclined 45° to the right, and the atoms revolving from right over to the left.

(6.) *The same*, and the axis the same, but the atoms revolving from left to right.

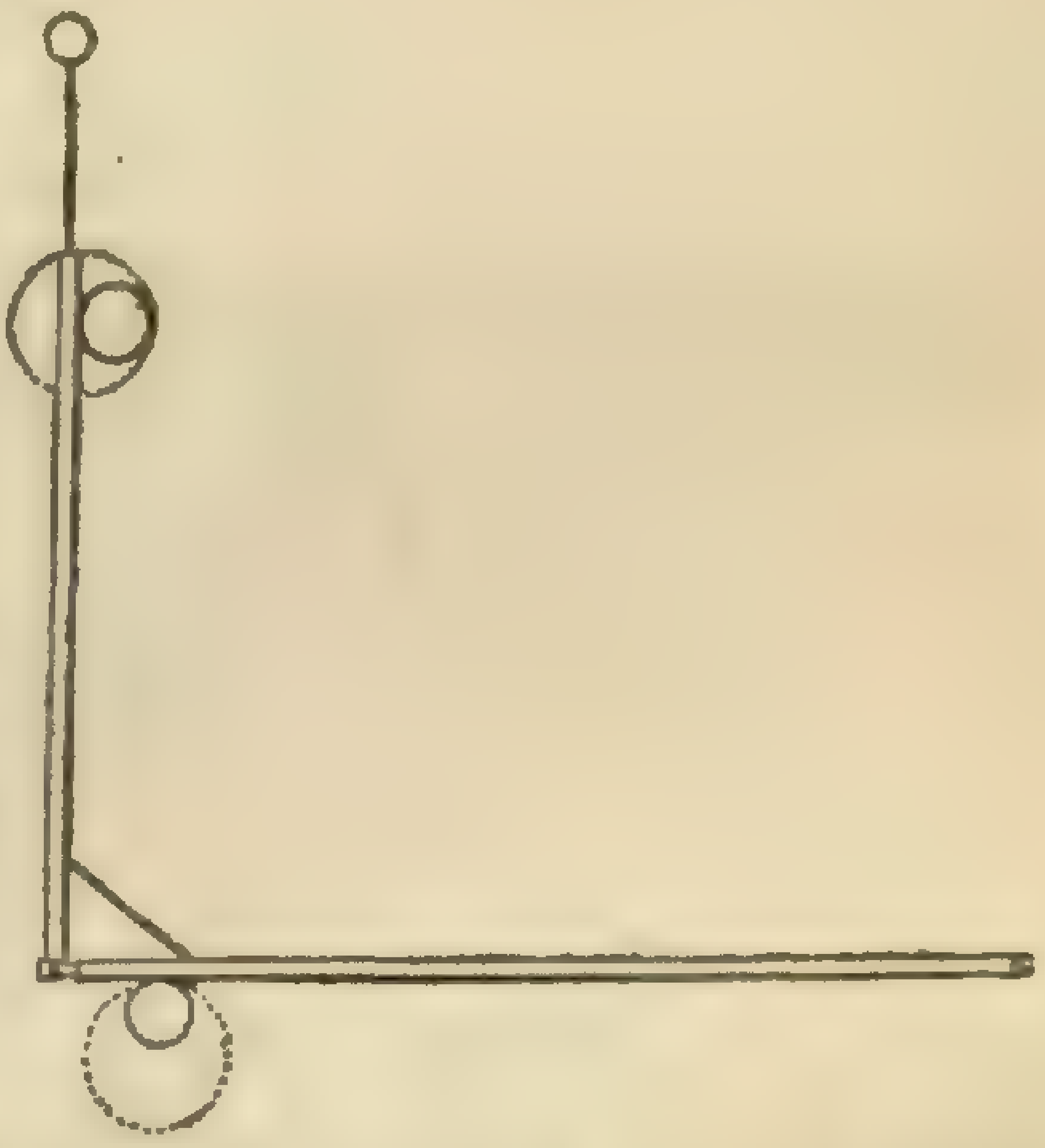
(7.) *The same*, the axis inclined 45° to the left, and the atoms revolving from right to left.

(8.) *The same*, and the axis the same, but the atoms revolving from left to right.

Moreover, in all the cases of elliptical polarization, the excentricity may vary to any degree, from the straight line on the one hand, to the circumference of a circle on the other.

It is unnecessary to state what is the relation of the cams of the two axes, for presenting all these varieties of polarization; to describe one or two is sufficient. Let a cam of the lower axis, Fig. 3, be at its highest position, and about to descend to the right, while the corresponding cam of the upper axis is farthest to the right, about to commence its motion to the left, as it descends; then the ball both *descends* by the first cam, and *moves to the left* by second, at equal rates; and therefore oscillates in a plane, which inclines upward 45° to the right. All the balls in regular succession do the same, so that the polarization is that of (1.) Again, change the relation of the axes, so that when one of the lower cams is at the highest point, the corresponding upper one shall be also at the highest point; then, when the ball *descends* by the first motion, it *moves to the right* by the second, till a quadrantal arc is described; then, continuing to descend, it moves to the left, till a semi-circumference is completed, and so on. This is *right-hand circular polarization* (4), provided the vibrations are propagated from particle to particle *toward the observer.* These and the other cases can be exhibited by the instrument in less time than is required to describe them.

3.



In one particular this instrument is inferior to Plucker's. In his, the balls in their component motions, strictly describe straight lines, in a vertical or a horizontal plane. But in this, these molecular motions, as exhibited by the balls, are obviously arcs of circles; consequently, all the compound motions deviate from the lines which have been mentioned. As the radius, however, is 18 inches, and the arc only about two inches, the curvature is not easily noticed. Since the whole purpose of the instrument is to produce motions which visibly agree with those determined by theory, in order to illustrate the subject to the mind of the pupil, the failure to fulfil the exact theoretical conditions is unimportant, while the mechanical advantages of this mode of producing the vibrations are obvious.

Amherst, Mass., Oct. 4, 1861.

ART. XLIII.—*Arsenic as an Impurity of Metallic Zinc;* by CHAS. W. ELIOT and FRANK H. STORER.

[*Remark:*—We now cite in full from their memoir on 'The impurities of Commercial Zinc' (*Mem. Am. Acad.*, vol. viii, 1860), the researches of Messrs. E. & S. on the interesting and often mooted question of the frequent presence of arsenic in commercial zinc. We thus redeem our promise given at the close of our former notice of this memoir (vol. xxxi, 147), and take pleasure in placing this fine research within easy reference by American readers. Its value in a medico-legal point of view cannot be too highly estimated.—EDS.]

The general opinion that arsenic is a very common impurity in commercial zinc may, we think, be traced back to the confident assertion of Proust, near the beginning of this century, afterwards extensively copied and generally believed. But it is quite clear that Proust, and probably many other of the early chemists, were led into error by the close resemblance of the sulphid of arsenic to the sulphid of cadmium, which last metal, not recognized till 1817, has since been shown to be a very common admixture in the zinc of commerce. The invention of Marsh's apparatus, in 1835, gave to chemistry a test for arsenic of most wonderful delicacy; and Marsh* himself, in his original memoir, describing his process, remarks, "that the only ambiguity that can possibly arise in the mode of operating above described, arises from the circumstance that some samples of the zinc of commerce themselves contain arsenic." But Marsh, thus careful to suspect his zinc, says not a word about the purity of his acid, and many observers since Marsh have been more ready to attribute the infinitesimal trace of arsenic, which his process has enabled them to detect, to the zinc, than to the acids they have used. Schauefele† has actually attempted to determine quantitatively the per cent of arsenic present in French, Silesian, and Vieille Montagne zincs, and his results have been quoted in many recent handbooks and treatises on toxicology.

The conclusions at which we have arrived, after a long course of experiments with many different zincs, and various acids, are these:—first, that much of the zinc of commerce is free from arsenic, or at least contains no arsenic that can be detected by the most delicate tests known for that metal; secondly, that the sulphuric and chlorhydric acids found in commerce do very often contain arsenic, and are always so liable to contain it as to be utterly unfit for use in Marsh's process without special purification for that purpose. The steps by which we were led to these results, and the evidence on which they are founded, we proceed to describe.

We have used exclusively Marsh's process for the detection of arsenic, applied with the apparatus and with all the precautions recommended by Otto.[*] Our apparatus consisted of a flask provided with a funnel-tube, and a tube bent at right angles, with which were connected by connectors of sheet India-rubber, first, a tube of the form of a chlorid of calcium tube, filled with asbestos; secondly, a similar tube, filled with pumice-stone soaked in caustic potassa; and thirdly, one filled with chlorid of calcium. Through these three tubes, in the order in which they are named, the gas generated in the flask was obliged to pass before it arrived at the reduction-tube, which was of hard German glass, and about one centimetre in diameter. The reduction-tube was drawn down to a fine bore, and during the progress of an experiment was heated by one of Bunsen's triple gas-burners. To prevent any elevation of the temperature in the flask during an experiment, it was immersed in cold water, and the dilute acid used was always cold, and added in small quantities. With this apparatus (which for convenience we shall designate as Otto's apparatus), taking every possible precaution to insure its perfect cleanness, we made several experiments upon Silesian zinc. 200 grammes of this spelter, carefully granulated, were used in each experiment, and the sulphuric acid employed was a commercial acid made in this country from Sicily sulphur. We were not unaware of the fact, that arsenic is almost invariably found in the foreign sulphuric acid made from various impure sulphurs of unknown origin, or from pyrites;[†] but it is a common impression that the American acid manufactured directly from Sicily sulphur is free from arsenic. Positive statements to this effect have been made by chemists who have had mainly in view the common use of sulphuric acid in the preparation of chemical compounds used in pharmacy, and the assertion has enough plausibility to command ready and general belief. Using such acid and 200 grammes of granulated Silesian zinc, we obtained, at the end of the hour during which the reduction-tube was heated, a deposit of arsenic perfectly distinct, though not bright enough to be called a mirror. Our next experiment was made with the same acid upon 200 grammes of an excellent sample of Vieille Montagne zinc, perfectly clean and carefully granulated. At the end of the hour during which the gas was passed through the reduction-tube, a brownish, volatile coating was found in that part of the tube where the bore was smallest. These experiments on Silesian and Vieille Montagne zincs were several times repeated, and always with the same result; the deposit in the reduction-tube was often too thin and slight to be positively identified as arsenic, but it could not be distinguished from the deposit of that metal, and would have been perfectly fatal in a medico-legal investigation, or in any case in which absolute purity of the materials was desired. Not convinced that the zincs were the source of the arsenic, we desired to prepare a quantity of sulphuric acid in which the presence of arsenic could not possibly be suspected. To attain this object, we subjected a specimen of American sulphuric acid to the following process. The acid was first boiled with a little flowers of sulphur, as proposed by Barruel,* in order to free it from the nitrous fumes which the common sulphuric acid almost always contains; a small quantity of pure chlorhydric acid was then stirred into the cooled acid, which had been carefully decanted from the free sulphur, and the whole again boiled; to the acid, again cooled, a second addition of chlorhydric acid was made, and again the acid was heated till dense white fumes had been escaping for upwards of half an hour. During this process, the volatile chlorid of arsenic is completely driven off, the second addition of chlorhydric acid being made, as has been recommended by H. Rose,† in order to insure this result. Lastly, a portion of chlorine-water was added to the cooled acid to oxydize any sulphurous acid which might be contained in it, and after a third boiling, the acid, cooled and diluted with three parts water, was ready for use. This method of purifying sulphuric acid is a combination and modification of several well-known processes. Buchner‡ has described in full the process of purifying sulphuric acid by means of chlorhydric acid, and the use of chlorine as above described was recommended by Jaquelain.§ The whole operation can be performed in a shallow evaporating-dish, and presents no serious difficulties of any kind. With the acid thus prepared, we tested 200 grammes of Vieille Montagne zinc, and after passing during more than an hour a continuous, gentle stream of gas through the reduction-tube, of which about four centimetres were maintained at a bright red heat, we found that there was absolutely no deposit whatever in the cool and narrow part of the reduction-tube. With the same acid and apparatus, 200 grammes of Pennsylvanian zinc (which had been proved to be altogether the purest zinc in our possession) gave absolutely no deposit of any kind in the fine reduction-tube at the end of one hour, the time during which, in all our examinations for arsenic, we maintained a steady flow of hydrogen through the red-hot reduction tube. We had now demonstrated that two different spelters, of which we were fortunate in possessing considerable quantities, were free from arsenic, or, more strictly, that in the given quantities of metal, and in the stated times, Marsh's infinitely delicate test could not detect arsenic in these two zincs. It was also rendered very probable that the sulphuric acid with which we first experimented contained arsenic, inasmuch as we had obtained a distinct deposit of arsenic from that acid and the Vieille Montagne zinc, which subsequent experiment had proved to be free from that impurity. In order satisfactorily to establish these conclusions, it was necessary to prove by frequent repetition that the same result might always be expected from these two zincs, and that their freedom from arsenic was a property shared by the whole sample, and not an accidental peculiarity of a particular fragment. At sundry times we therefore repeated again and again the long and careful test for arsenic above described with these two samples of spelter, and invariably arrived at the same conclusion; namely, that no deposit of any kind could be obtained in the reduction-tube from these zincs and purified sulphuric acid.

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* Edinburgh New Phil. Jour., xxxv, 235.

† Extract from a thesis presented by M. Schauefele. Jour. de Chimie Médicale, [3], vi, 173; also in Dingler's Polyt. Jour., 1850, cxvi, 248.

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[*] A Manual of the Detection of Poisons. Translated from the German by Elderhorst. New York: Baillière. 1857.

[†] On the subject of arsenic in foreign sulphuric acid, the following authorities may be referred to:—Martius, Schweigger's Jour. f. Ch. u. Phys., 1811, iii, 363. Wackenroder, Ann. der Pharm., 1834, xii, 189. Ib., 1835, xiii, 241. Vogel, Jour. f. pr. Ch., 1835, iv, 239. Ficinus, Ann. der Pharm., 1835, xv, 77. Berzelius, in his Jahresbericht, 1841, xx, 192. Brett, Philosophical Mag., 1842, [3], xx, 404. Schnedermann and Wöhler, Jour. f. pr. Ch., 1845, xxxv, 186. Dupasquier, Comptes Rendus, 1845, xx, 794. Cameron, Chem. Gazette, No. 320, p. 75, in Jour. f. pr. Ch., 1856, lxviii, 64.

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* Dingler's Polyt. Jour., 1837, lxiv, 55; from Jour. de Ch. Médicale, 1836, No. 4.

† Pogg. Ann., 1858, cv, 571.

‡ Ann. der Ch. u. Pharm., 1855, xciv, 241.

§ Ann. de Ch. et Phys., 1843, [3], vii, 191.

Delicacy of the Test.—To prove the sufficiency of our apparatus, and the absence of every substance which might be supposed to prevent the formation of the arsenic mirror, and, moreover, to obtain mirrors from known quantities of arsenic with which roughly to compare deposits obtained in experiments in which the arsenic was the unknown quantity, we made the following experiments:—

1. 200 grammes of Vieille Montagne zinc, about 200 c. c. of purified dilute sulphuric acid, and 20 drops of pure chlorhydric acid, were first thoroughly tested for arsenic, and found to be perfectly pure.

2. 200 grammes of Vieille Montagne zinc, and about 200 c. c. of purified sulphuric acid, were thoroughly tested, and gave no deposit in the reduction-tube. Into the flask whose contents had been thus proved, two tenths of a milligramme of arsenious acid (weighed on a Plattner's assay balance, made by Lingke of Freiberg), dissolved in 20 drops of the same chlorhydric acid used in the first experiment, were introduced. An enormous mirror of arsenic appeared instantly in the reduction-tube. To get the greatest effect, the arsenious acid should be thoroughly dissolved, and its solution should be effected without the use of any but a very gentle heat.

3. Using the same zinc and the same acids, in the same quantities which were employed in the foregoing experiments, and proving the materials as in the last experiment, we obtained in half an hour, a very large and distinct mirror of arsenic, by introducing into the flask one tenth of a milligramme of arsenious acid.

4. 200 grammes of Pennsylvanian zinc and about 200 c. c. of purified sulphuric acid were tested for one hour, and proved to be perfectly pure. One milligramme of arsenious acid had been dissolved in 20 drops of the pure chlorhydric acid which had been used in the first experiment, and the solution diluted with distilled water to the bulk of 50 c. c. One cubic centimetre of this solution was introduced into the flask whose previous contents had been proved as above described, and at the end of three quarters of an hour a distinct deposit of arsenic was found in the reduction-tube. The amount of arsenious acid actually placed in the flask was two hundred-thousandths of a gramme (.00002 gram.).

$$0{\cdot}00002 \text{ grm. arsenious acid} = 0{\cdot}000015 \text{ grm. arsenic.}$$

$$\text{Ratio of the arsenic present to the zinc} = 0{\cdot}000015 : 200 = \frac{75}{1{,}000{,}000{,}000}.$$

$$\text{" \quad " \quad " \quad " \quad " amount of liquid in the flask} = \text{about } \frac{75}{1{,}000{,}000{,}000}.$$

Our apparatus was therefore competent to detect a quantity of arsenic less than one ten-millionth of the weight of the zinc used, or of the amount of fluid in the flask. This quantity of liquid necessarily varied somewhat, in consequence of the slight additions of acid necessary to maintain a constant current of hydrogen, but only varied to be increased, never diminished. Remembering the wide limits of error in many chemical processes, the comparative coarseness of most chemical tests, and the many unavoidable inaccuracies in weighing and measuring, is not the assertion perfectly safe, and in strict conformity with the general use of language in other qualitative or quantitative determinations, that a specimen of zinc is free from arsenic, which does not show the slightest trace of that metal in an apparatus abundantly capable of detecting the ten-millionth part of arsenic? We are aware that this is not the limit of delicacy[*] of Marsh's test, but, assured of this delicacy, we rest satisfied with it as sufficient for our present purpose. The more delicate the test, the stronger is our present argument, and the greater need is there of exceeding caution in applying this test in the investigations of medico-legal or pharmaceutical chemistry.

In connection with these experiments on the delicacy of the test, we would call attention to the fact, that the sulphuretted hydrogen, which we have shown in our examinations for sulphur to be always developed from commercial zinc, does not prevent the exhibition of such a very small amount of arsenic as 0·000015 gram. Chevallier,[†] and more recently Blondlot[‡] and Leroy,[§] assert that the presence of the insoluble sulphid of arsenic cannot be recognized by Marsh's test, and that arsenic may therefore be concealed by being converted into the sulphid. This is the natural and general opinion, though Marsh, in his original memoir, distinctly says, that "the presence of arsenic in artificial orpiment and realgar, ... and in sulphuret of antimony, may be readily shown by this process, when not more than half a grain of any of those compounds is employed."[‖] When the amounts of arsenic and of sulphuretted hydrogen are alike minute, it is quite certain that the reaction for arsenic is not affected by the unavoidable presence of this gas.

Arsenic in American Acids.—We have tested four different kinds of American sulphuric acid, of which two were commercial oil of vitrol, and two were sold as chemically pure acids. The test applied to these acids was always the same, and may be described once for all: 100 grammes of Pennsylvanian zinc, from the same bar which in many trials had been shown to be free from arsenic, was placed in the flask, and the acid to be tested was used instead of the purified acid which was always employed when the zinc was the suspected substance. In every experiment, the gas was passed through the reduction-tube at least one hour. In experiments several times repeated, the sulphuric acid made at Providence, Rhode Island, invariably yielded a distinct deposit in the narrow part of the tube. It should be stated, however, that the Providence acid used in these repeated experiments all came from one carboy. In a sample of the acid made at Waltham, Massachusetts, we detected a similar trace of arsenic. The deposits obtained from these two acids were hardly larger than that produced by the 0·000015 gram. of arsenic used in the fourth experiment on the delicacy of the reaction, but, on the other hand, only a small quantity (from 25 to 50 c. c.) of the acid could be employed in a single experiment.

The arsenic which is eliminated from these acids during the process of purification with chlorhydric acid may easily be collected, and exhibited by Marsh's test. For this purpose, the sulphuric acid should be heated with the chlorhydric acid in a flask or retort, from which the gas generated is conducted into a small quantity of distilled water, kept constantly cool. The volatile chlorid of arsenic condenses in the water, and the arsenic in the solution is readily manifested in Otto's apparatus. The chlorhydric acid used in this experiment must be absolutely free from arsenic; such acid may be obtained by passing sulphuretted hydrogen through chlohydric acid prepared from salt and pure sulphuric acid. Moreover, the sulphuric acid in the flask or retort must be kept fuming during at least half an hour, in order to secure the complete volatilization of the chlorid of arsenic.

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[*] M. Signoret (Taylor on Poisons, 2d Edition, 1859, London, p. 396) states that he has procured metallic deposits with only the 200,000,000th part of arsenic in the liquid; but it is not clear from such a statement what the exact amount of arsenic in the apparatus was which enabled him to obtain deposits,—a very material point.

[†] Jour. de Ch. Méd., [2], v, 380, in Berzelius's Jahresbericht, 1841, xx, 192.

[‡] Comptes Rendus, 1857, xliv, 1222.

[§] Ibid., 1859, xlix, 469.

[‖] Edinburgh New Phil. Jour., xxxv, 285.

The chemically pure sulphuric acid, so called, manufactured by Rosengarten, of Philadelphia, was purer than the commercial acid with reference to arsenic as well as to lead; indeed, in one experiment it yielded no sensible deposit in the reduction-tube, but in several subsequent experiments with pure zinc, in which we attempted to use Rosengarten's acid instead of that purified by ourselves, we obtained faint deposits which precluded its use, and showed it to be untrustworthy in such delicate examinations for arsenic by Marsh's process. To the "chemically pure" sulphuric acid made by Powers and Weightman of Philadelphia, precisely the same remarks apply; it is unfit for use in any research where scrupulous accuracy is necessary, and of which the results are worse than worthless if they be not certain and impregnable. It may be granted that the amount of arsenic in any small quantity of these acids is really too minute to be of any consequence, except in the most refined experiments. On the other hand, it must be remembered that in some pharmaceutical processes, and in many chemico-legal examinations for arsenic, in which a large amount of acid is often necessarily used, the whole of the arsenic contained in the reagents employed is, by the very nature of the process, concentrated and condensed into a very small compass. For example, in a poisoning case in which the chemist is obliged to destroy by acids any considerable portion of the body, as is often the case, it may be necessary to use many pounds of sulphuric or chlorhydric acid, and the very care and pains with which the chemist labors to concentrate every grain of arsenic in that organic matter into the small glass of liquid, which he finally tests by Marsh's process, also concentrates into the same glass all the arsenic contained in all the reagents which he has employed in the whole process. Under such circumstances the existence of *any* arsenic in sulphuric acid, capable of exhibition from a few cubic centimetres of the acid, becomes a fact of the utmost moment. From neglecting this arsenic in sulphuric acid arose the long controversy concerning normal arsenic in the animal body. To the objection that Marsh's test is too delicate, and that we should find all the elements everywhere if we had for each of them a test as refined as Marsh's for arsenic, it may be replied, first, that Marsh's process is not thought too delicate to base vital conclusions upon in difficult examinations for arsenic in poisoning cases, and secondly, that facts are not to be met by a theoretical objection, which is at any rate purely speculative, and furthermore is no objection if, as is certainly possible, the theory be true.

We are aware of the common opinion, that sulphuric acid made from Sicily sulphur contains no arsenic,[*] and we do not propose to explain the source of the arsenic found in American sulphuric acid, further than to suggest that its presence seems not unnatural when we remember that the sulphides of arsenic are often associated mineralogically with the sulphur from which the acid is made. Long ago Pfaff,[†] in commenting upon the observation made by Martius of arsenic in sulphuric acid, said that the sample analyzed by Martius was probably made from sulphur containing orpiment or realgar, minerals which are found with sulphur in the solfataras. Stromeyer[‡] detected arsenic in the mixture of sal-ammoniac and sulphur, which is one of the many volcanic products of the Lipari Islands, and there seems to be no good reason for supposing that the sulphur, which is exported from that locality, would escape contamination with arsenic. Daubree[§] has remarked that arsenic, as sulphid, occurs in the fissures of the lavas at Etna, at Vesuvius, and at the solfataras of Pouzzoles and of Guadeloupe. Scacchi[‖] also states that among the substances found in the fumaroles of the solfataras are pyrites, realgar, mispickel, and dimorphine. Orfila,[¶] and before him Vogel, of Munich, imply that the leaden chambers in which sulphuric acid is made communicate arsenic to the acid.

Whatever we have said with regard to the American sulphuric acid, applies with still greater force to the commercial chlorhydric acids. That common chlorhydric acid contains chlorid of arsenic, is a fact which was long ago observed, and has been fully discussed by Wackenroder,[**] Dupasquier,[††] Otto,[‡‡] and many others.[§§] We have examined two different samples of chlorhydric acid made in this country. Dilute chlorhydric acid, instead of sulphuric, was used in Marsh's apparatus with 200 grammes of pure Vieille Montagne zinc, and before the hydrogen generated had been passing through the heated reduction-tube fifteen minutes, there appeared in the fine part of the tube a brown deposit, which in an hour increased to a large and distinct mirror of arsenic, readily verified by other tests. Both samples of acid gave the same result, and we may add that, even on a small scale, we found great difficulty in preparing from salt and sulphuric acid a specimen of chlorhydric acid perfectly free from arsenic. The thorough purification of the acid by means of sulphuretted hydrogen, as recommended by Otto, is therefore an absolutely necessary preliminary to the use of chlorhydric acid in an examination for arsenic.

From the examination of so few samples of sulphuric and chlorhydric acids, we do not pretend to have established the affirmative proposition, that there is always arsenic in these acids; their impurity was only an incidental difficulty in this research, and we strayed thus far from our main subject, only because of the great importance of trustworthy information upon this point to the pharmacist, and to the chemist who has to do with poisoning cases. Our observations, in connection with the facts long since established regarding the contamination of foreign sulphuric and chlorhydric acids with arsenic, may well lead the pharmacist and the analytical chemist to distrust his acids, till accurate experiments have proved them to be above suspicion; and we believe that careful investigations will hereafter show that arsenic is introduced into pharmaceutical preparations by the acids employed in their manufacture to an extent far greater than would now be credited. The task of the chemist who is called upon to examine a human body or organs of the body for arsenic, is a simple one when the poison is found in its original condition, unabsorbed and unaltered, but in difficult investigations of this kind, when the poison has been absorbed and diffused through a large mass of organic matter which must be destroyed by acids, the precautions insisted upon by Gaultier de Claubry,* and by Galtier,† and other modern toxicologists, should be strictly observed. Not only should all the reagents to be employed be thoroughly tested *à blanc*, but furthermore, an experiment parallel with the actual examination of the suspected organic substances should be carried on with the same reagents in the same quantities, in a similar apparatus, and in all respects under like conditions, upon a quantity of normal animal matter equal to the weight of the suspected substances. In this way only can the chemist avoid the fatal uncertainty consequent upon the employment, in large quantity, of reagents whose purity is not above suspicion.

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[*] For a strong statement of this opinion, see Ure's Dictionary of Arts, &c., 4th edition, Boston, 1853, vol. ii, pp. 791, 799.

[†] Schweigger's Jour. f. Ch. u. Phys., 1816, xviii, 283.

[‡] Ibid., 1825, xliii, 452.

[§] Ann. des Mines, [4]. xix, 680.

[‖] Journal f. pr. Ch., 1852, lv, 54.

[¶] Ann. d'Hygiène Publique, xxii, 408.

[**] Ann. der Pharm., 1835, xiii, 241.

[††] Comptes Rendus, 1841, xiii, 630.

[‡‡] Ann. der Ch. u. Pharm., 1856, c, 39.

[§§] See also Ure's Dictionary of Arts, &c., 4th edition, (Boston, 1853,) ii, 248, Art. *Muriatic Acid.*

We return to the examination of other zincs for arsenic. With the same purified acid used in our previous experiments on Pennsylvanian and Vieille Montagne zincs, we tested 200 grammes of *Silesian* zinc, carefully granulated, and perfectly clean. For half an hour, the hydrogen passed steadily through the red hot reduction-tube without leaving the slightest deposit in the fine tube beyond the heated portion, but on continuing the operation beyond this time (indicated by Otto as sufficient for the testing of the materials *á blanc*), a faint but perceptible mirror gradually formed. This result indicates, first, that this sample of Silesian zinc was not perfectly free from arsenic, and secondly, that it is dangerous to conclude that the zinc and acid, which have given no reaction for arsenic during half an hour in Otto's form of Marsh's apparatus, will therefore give no mirror in the next half hour, even though no arsenical compound be added to the apparatus. In any delicate examination for arsenic, this is a point to be carefully borne in mind. Our results with this zinc were corroborated by several similar experiments.

The zinc of Rousseau Frères was next submitted to the same test, with the same acid: 75 grammes of this zinc yielded in half an hour a brown deposit, hardly to be called a mirror, although covering a considerable portion (two centimetres) of the tube. The final result of the experiment was not distinguishable from the result of the test of Silesian zinc.

With the same acid and apparatus, 200 grammes of New Jersey zinc gave a distinct mirror of arsenic, so large in amount, that the arsenic could be easily recognized by its characteristic odor. The mirror began to form at once, and gradually increased during the hour, which was the duration of the experiment.

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* Briand et Chaudé, et Gaultier de Claubry, Manuel complet de Médecine Légale, 5me édition, (Paris, 1852,) p. 752.

† C. P. Galtier, Traité de Toxicologie, (Paris 1855,) Tom. i, p. 362.

Having at hand a quantity of the ore from which this zinc is extracted, we extended our search for arsenic to the red oxyd of zinc, which is the source of this spelter. Several grammes of the red oxyd, finely powdered, were moistened with 30 drops of pure nitric acid, and treated with a measured quantity of pure chlorhydric acid, prepared from common salt and sulphuric acid free from arsenic. The solution, with the very slight residue, was then gently evaporated to a small bulk, with a small measured quantity of Rosengarten's sulphuric acid. The imperfect solution thus prepared was introduced into the flask of our apparatus, whose previous contents of zinc and acid had been thoroughly tested for one hour, and found perfectly pure. In ten minutes a distinct deposit of arsenic was obtained, which in half an hour increased to a large and unmistakable mirror. To prove beyond a doubt that this arsenic came from the oxyd of zinc, and not from the acids employed in preparing the solution, we tested in a clean apparatus with fresh zinc and acid, which had been proved pure by a test of one hour in duration, the same quantities of the same acids evaporated together as in the experiment above described. At the end of one hour (the second hour during which the apparatus had been at work) a deposit, perceptible on close inspection, was discovered in the narrow part of the reduction-tube. This deposit was invisible on any cursory examination, and bore no comparison with the very decided mirror of arsenic obtained in the previous experiment. In two other similar examinations of the red oxyd of zinc, we obtained the same strong evidence of the presence of arsenic in this ore, and the associated mineral, Franklinite, yielded, in the single careful test to which we subjected it, a mirror of arsenic sufficient to give the smell and all the other characteristic reactions for arsenic. To obtain satisfactory results, the solution of the oxyd must never be heated above 100°, and the small quantity of nitric acid which is used to facilitate the solution must be completely driven off before the liquid is introduced into the apparatus.

If any further evidence of the presence of arsenic in the New Jersey spelter and its ore were needed, it might be found in the following experiment with a zinc which we ourselves prepared by reducing the New Jersey white oxyd of zinc with charcoal, in a refractory retort such as are furnished by the dealers in chemical apparatus at Paris: 20 grammes of this zinc, tested in Otto's apparatus with purified sulphuric acid, yielded in five minutes a distinct deposit of arsenic, and in half an hour a large mirror.

To ascertain whether the Pennsylvanian and Vieille Montagne zincs were always free from arsenic, we procured and tested another sample of the zinc manufactured at the Pennsylvanian and Lehigh zinc works, and a second sample of Vieille Montagne spelter. The Pennsylvanian zinc was, as before, remarkably free from lead, leaving no residue when dissolved in dilute sulphuric acid; but on testing 200 grammes of it in Otto's apparatus with purified acid, it gave in half an hour a slight deposit in the reduction tube, which in an hour increased to a distinct brown coating. A similar result we obtained in testing the second sample of

Vieille Montagne zinc; 200 grammes of it with pure acid yielded a deposit in the reduction-tube, fatal to its use in any delicate experiments. It should be stated, that the external appearance of this spelter indicated very clearly that its quality was inferior to that of the sample first examined. It is obvious from these results, that zinc manufactured in the same works, and from the same ore, may not always contain the same impurities, or rather that it is never to be expected to contain the same percentage of the same impurities. From the nature of the process of reduction, it would naturally be the case that the more volatile impurities should be present in the zinc which distils first in greater quantities than in the zinc which is reduced from the last part of a given charge of ore. Thus it is easy to imagine that the zinc which comes over first should be contaminated with arsenic, while that which is last reduced might be perfectly free from that impurity. The same principle explains the variations in the amount of cadmium contained in different samples of the same spelter, and indeed accounts in great measure for the varying per cents of all the impurities found in different specimens of any zinc, though made by the same process from the same ore.

Lastly, we submitted our four specimens of English zinc to the test for arsenic. With purified acid, 200 grammes of the zinc made by Dillwyn and Company began to show a deposit in the reduction-tube within ten minutes of the commencement of the experiment, and in twenty-five minutes this deposit increased to a very perceptible mirror. A similar result was obtained from 200 grammes of the Mines Royal zinc. In fifteen minutes the stream of hydrogen from this zinc began to deposit arsenic in the reduction-tube, and at the end of three quarters of an hour, a thin but perfectly distinct mirror extended over three or four centimetres of the fine tube. On the label which accompanied the specimen of the spelter made at Minera, near Wrexham, it was stated that the zinc was manufactured from silicate of zinc, and we therefore expected to find this spelter purer than the ordinary English zinc made from blende; but, on the contrary, it contained a large amount of lead, and the test for arsenic showed it to contain more of that impurity than either of the two specimens before examined: 100 grammes of the Wrexham zinc began to show a deposit in the reduction-tube in ten minutes from the beginning of the experiment, and at the end of an hour a mirror had accumulated large enough to be identified by the arsenical odor. But of the four samples of English zinc, that of Messrs. Vivian contained the most arsenic; 200 grammes of this spelter yielded an enormous mirror of arsenic in less than ten minutes, and in a few minutes more a second mirror, large enough to give the characteristic odor. With regard to English zinc, therefore, our observations do not agree with those of Brett,* who states that he has examined many specimens of English and foreign zincs, and could never obtain any indications of arsenic when the sulphuric acid was pure. The explanation of this discrepancy is to be found in the fact, that the test applied by Brett, although essentially Marsh's test, had not the extraordinary delicacy which is insured by Otto's form of Marsh's apparatus.

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* Philosophical Magazine, 1842, [3], xx, 404.

This question now suggested itself: In presence of an excess of zinc, is not arsenic retained in the black residue (lead) from zinc dissolved in dilute acids, in such a condition, that it is not attacked by the acids or by the hydrogen, and therefore escapes detection? To determine this point, if possible, we dissolved 40 grammes of the Vieille Montagne zinc, which had showed no trace of arsenic by Marsh's test in dilute sulphuric acid, free from arsenic. The residue obtained was washed, treated with chlorhydric and a few drops of nitric acid, and the solution gently evaporated to a small bulk in presence of a little pure sulphuric acid. The mixture thus obtained was washed into the flask of Otto's apparatus, whose previous contents of zinc and acid had been thoroughly tested *à blanc* and found pure. At the end of the second hour, a very slight deposit was discernible on close inspection in the fine tube, but the result was too doubtful to warrant the assertion that arsenic was contained in the insoluble residue. We next tried the same experiment with Silesian zinc, in which Marsh's test had detected arsenic. The residue from 40 grammes of this zinc, submitted to the process just described, produced in the reduction-tube in fifteen minutes a brown deposit, which in half an hour became well marked, and at the end of the hour afforded sufficient evidence of the presence of arsenic in the residue, it being well understood that the zinc and acid used in this experiment had been previously tested for one hour and found pure. The only conclusion to which these experiments point is, that when a given sample of zinc contains arsenic, a portion of that arsenic will escape combination with the hydrogen generated by the solution of the zinc, and will remain in the insoluble residue. It was useless in this connection to examine the residue from New Jersey zinc, because that spelter itself contained arsenic, and its residue contained metallic copper, which would inevitably retain arsenic, as in Reinsch's test. The Pennsylvanian zinc gave no residue with acids.

The absolute necessity of obtaining zinc free from arsenic for many chemical investigations, renders any process for purifying zinc from arsenic a matter of considerable interest and importance. We therefore tried the process of purifying zinc by fusing it with one fourth of its weight of saltpetre, a method fully described by Meillet,* but previously suggested by Orfila:† 760 grammes of Silesian zinc were finely granulated and mixed with one fourth this weight of saltpetre, by placing the zinc and nitre in alternate layers in a Hessian crucible. The mixture was heated till deflagration ensued, when the melted mass was poured into cold water to separate the slag, caustic potassa, and any arseniate of potassa which might have formed. The washed mass was remelted, and again granulated. The loss of zinc during the process is very large; we obtained only 200 grammes from the original 760 grammes. Of this zinc 170 grammes were tested in Otto's apparatus with pure acid, and in twenty minutes there began to form in the reduction-tube a brownish deposit, which, at the end of three quarters of an hour, was a sufficient evidence of the presence of arsenic. The deposit was as large as that obtained from the same Silesian zinc, before it had undergone this process of fusion with nitre. It has already been stated, under the appropriate head, that this process did not disembarrass the zinc of the sulphur which it contained, and there seems to be little reason for expecting the complete removal of the arsenic, inasmuch as the fused saltpetre can only be brought in contact with the external surface of the zinc, however finely the metal may be granulated. It is not inconceivable that a trace of arsenic in a zinc should be eliminated by Meillet's process, and that a sample, originally almost absolutely free from arsenic, should be so improved as to afford no perceptible mirror; thus Stein* could not detect arsenic in a sample of zinc purified by this method, but as a general rule it will not be safe to rely upon this process for the conversion of arsenical commercial zinc into zinc fit for use in Otto's apparatus.

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* Dingler's Polyt. Jour., 1842, lxxxiii. 205; from Jour. de Pharmacie, 1841, p. 625.

† Annales d'Hygiéne Publique, 1839, xxii, 427.

On this subject of arsenic in commercial zinc two opposite errors demand notice. On the one hand not a few chemists have maintained that commercial zinc almost invariably contains arsenic, and that Marsh's test is untrustworthy on this account. Thus Persoz † states that the greater part of the zinc sold in Strasbourg contains arsenic, and in a previous paper,‡ condemning Marsh's process, he remarks with truth, that even distilled zinc may give spots of arsenic. The opinion that all zinc contains arsenic, finds support in the quantitative determinations by Schauefele of the amount of arsenic in French zinc, Selesian zinc, and Vieille Montagne zinc respectively. These determinations have been quoted in almost all the modern text books, and have had in our opinion much more weight than they are really entitled to. Schauefele determined the arsenic in his samples of zinc by two methods. The first was that of Villain, and consisted in counting the number of arsenic spots obtained from a given weight of zinc, and estimating the corresponding amount of arsenic by means of the following absurd rule:—one milligramme of arsenious acid will give two hundred and twenty-six arsenical spots two millimetres in diameter. The utter unfitness of this process for exact experiments is too obvious to need any illustration. In applying this singular method, Schauefele completely dissolved one kilogramme of zinc in dilute *sulphuric acid*, but in this connection says not a word about the purity of the acid, of which he must have used at least one kilogramme and a half. The second method used by Schauefele was essentially that described by Jacquelain, and consisted in passing all the hydrogen generated by a given weight (from 320 to 800 grammes) of zinc through a solution of chlorid of gold; this solution was partially decomposed by the arseniuretted [sulphuretted ?] hydrogen, and when the zinc had been completely dissolved, the chlorid of gold solution, which was supposed to contain all the arsenic of the zinc in the condition of arsenious acid, was further decomposed as completely as possible by means of *sulphurous acid*, and the precipitated gold separated by filtration. In the filtrate there still remained a small amount of chlorid of gold, which had escaped reduction by the sulphurous acid, and to separate this chlorid from the solution of arsenious acid, distillation was resorted to. The retort in which the residue from the distillation remained was washed out with water acidulated with *chlorhydric acid*, and the liquid so obtained was added to the original distillate, through which sulphuretted hydrogen was then passed. The precipitated sulphid was collected on a filter, dried, and redissolved in *ammonia;* this ammoniacal solution was then evaporated to dryness, and the residue weighed as sulphid of arsenic. As the result of this complicated process, which involves the use of so many different reagents and vessels, Schauefele obtained a quantity of arsenic which varied between two thousandth of one per cent of the weight of the original zinc, in that sample which contained the most arsenic, and two five-thousandths of one per cent in that which contained the least. The weight of the sulphid of arsenic, which was the final result of the analysis, in no case exceeded ten milligrammes. There is no certainty that this very small amount of arsenic was not derived from the acids used in the process, for Schauefele merely states that his sulphuric acid, of which he used a very large quantity, was "absolutely free from arsenic," and the other reagents are said to be pure. The methods by which he insured this absolute purity are not even hinted at, and we therefore have no opportunity of judging for ourselves of the sufficiency of the processes employed to eliminate the arsenic from these reagents, of which two at least almost invariably contain appreciable amounts of that impurity. Quantitative determinations, in which the original weights are kilogrammes, and the final weight milligrammes, are trustworthy only when the processes are short and simple, and the reagents employed are proved to be above suspicion. The process of M. Schauefele was long and complex, and his reagents, so far from being proved to be above suspicion, were in all probability the source of the arsenic which he attributed to the zinc. In the valuable paper which we have before cited, Karsten distinctly states that the Silesian zinc is free from arsenic, basing this statement upon experiments which in their general method closely resembled those of Schauefele; he endeavored to decompose a solution of nitrate of silver by a stream of hydrogen generated by the zinc under examination, but though his process was analogous to that of Schauefele, he was led to the diametrically opposite conclusion. Again, Schauefele's second method was essentially the process which has been thoroughly studied by Jacquelain, who claims for it a delicacy* equal to that of Marsh's process; and yet in Jacquelain's own hands this method detected no arsenic in the specimen of zinc which he examined.† We cannot avoid the conclusion, that the determinations given in M. Schauefele's thesis have no general significance, and have gained more credit than they deserve. Our observations conclusively prove that there are zincs in commerce which are not contaminated with arsenic, and it should be noticed that, while one of our pure samples (the Pennsylvanian) was of a zinc which is not yet manufactured in large quantities, the other was a specimen of the Belgian zinc, one of the most common and abundant of the commercial spelters.

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* Jour. f. pr. Chem., 1851, liii, 40. † Ann. de Ch. et Phys., [3], 1844, x, 507, note.
‡ Ibid., [2], 1840, lxxiv, 432.

We turn now to the discussion of the opposite error, namely, that arsenic is very rarely to be found in the zinc of commerce. On this point we need only quote the strong statements of the highest authorities. Regnault, in the report‡ to the French Academy on Marsh's process and its modifications, wrote : "It is easy to procure in commerce zinc and sulphuric acid which gives no arsenic in Marsh's apparatus." With proper understanding with what is here meant by "Marsh's apparatus," this statement is as true now as it was twenty years ago. The committee relied chiefly upon the production of arsenical spots on porcelain, and though they recommended a form of apparatus adapted for heating the arseniuretted hydrogen to redness, yet in this apparatus the reduction-tube was not drawn down to a fine bore beyond the heated portion of the tube, and the committee in their own experiments seem to have preferred the arsenical spots as affording the best evidence of the presence of arsenic. They completely dissolved 500 grammes of commercial zinc in dilute sulphuric acid, and obtained from the hydrogen evolved no sensible arsenical spot; the black residue they did not examine. The test to which we have submitted our acids and zincs is more delicate than that applied by the committee of the French Academy; Otto's apparatus is more sensitive than that used by this committee, and will detect the presence of arsenic in quantities too small to produce sensible spots. It is self-evident that the continuous deposition of arsenic from a stream of hydrogen as it flows steadily through a very fine tube for an hour or more, would exhibit an amount of arsenic too minute to give the slightest perceptible spot in the instant during which the porcelain surface is held in the burning jet of gas. The first reaction is prolonged and accumulative, the second is intermittent and instantaneous. Blancard,* in commenting upon the statement regarding the ease of obtaining pure zinc, which is above quoted from the Report, has remarked with truth, that many zincs of commerce which give no *spots* by Marsh's apparatus, nevertheless contain sometimes antimony, sometimes arsenic, sometimes both.

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* Ann. de Ch. et Phys., [3], ix, 490. † Ibid., 1843, [3], vii, 189.

‡ Comptes Rendus, 1841, xii, 1076, and Ann. de Ch. et de Phys., [3], ii, 159.

The same explanation should accompany the statements of Orfila, with regard to arsenic in commercial zinc and acids. This distinguished toxicologist, in a very valuable paper on "The Means of being assured of the Presence of Arsenic," after remarking† that the sulphuric acid and the zinc of commerce sometimes contain arsenic, nevertheless implies‡ that he has never obtained decided arsenical spots on porcelain from a commercial sulphuric acid, and subsequently makes this explicit declaration: "I declare that I have made this experiment (the test for arsenic by the production of arsenical spots) more than five hundred times with the sulphuric acid and zinc of commerce, bought of various manufacturers of chemical products, and have only found arsenic *three times*."§ Our own experiments confirm the truth of this statement; of all the specimens of zinc which we have examined, not more than two contained enough arsenic to give spots on porcelain, and not a single sample of our sulphuric acid would have afforded that reaction for arsenic. So long as the chemist, employed upon a poisoning case, sought for arsenical spots alone, the little arsenic which his zinc might have contained could do no harm; it is only when important conclusions are to be drawn from more refined experiments, with a more delicate apparatus, that the arsenic so often present in zinc and acids becomes a matter of a very serious concern. Our experiments prove that arsenic is contained, not in all samples of commercial zinc, but in a large majority of them; and it will be perceived that we arrive at this result without impugning in the slightest the accuracy of the experiments upon which the very distinguished chemists whose words we have above quoted based opposite conclusions. * * * *

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* Dingler's Polyt. Jour., 1841, lxxxii, 425, from Jour. de Pharmacie, Sept., 1841, p. 543.

† Annales d'Hygiène Publique, 1839, xxii, 404. ‡ Ibid., 411. § Ibid., 424.

The purest of all the zincs which we have analyzed is that manufactured at the Pennsylvania and Lehigh Zinc Works, Bethlehem, Pennsylvania. This spelter dissolves in dilute sulphuric acid without leaving any appreciable residue, and therefore contains no lead; indeed a trace of cadmium is the only impurity whose presence in this zinc we could confidently assert. The ore from which this spelter is made is the hydrated silicate of zinc (electric calamine), and it is not surprising that this mineral should yield zinc of singular purity, if the ore be carefully selected. We have stated above that our first sample of this zinc was free from arsenic, but that the second sample was not pure in this respect. At these works the oxyd of zinc is manufactured, as well as the metal, and we learn from a letter addressed to Professor Brush by Mr. Wharton, the director of the works, that the crust from the oxyd furnaces has now and then been worked into spelter, and that the ore used in making the oxyd is less carefully selected than that which goes to the spelter furnaces, and is much more likely than the latter to contain both blende and pyrites. This fact may account for the occurrence of arsenic in some specimens of this spelter, while the greater part of it, manufactured from carefully selected silicate of zinc, is perfectly free from that impurity. There seems to be no reason why zinc of uniform purity should not be obtained from this excellent ore.*

Pure Zinc.—We have already referred to the difficulty of obtaining a pure zinc by reducing it from the oxyd on a small scale; nothing but the direst necessity could induce us to again attempt the operation, although it has been recommended by many high authorities. Nevertheless it is by no means difficult to prepare a pure oxyd of zinc, and manufacturers of pure chemicals, working on a larger scale than it is practicable for the chemist to do, might undoubtedly prepare from this oxyd at moderate cost a really pure zinc. There are some processes of chemical analysis in which the contamination of zinc with metallic lead is a matter of importance, because it affects the accuracy of the results; but in these cases the difficulty can generally be avoided by discarding zinc altogether, and resorting to other methods of analysis. But in order to use Marsh's process for the detection of arsenic, the chemist must procure zinc free from arsenic, and there is no escape from this necessity; redistillation will not disembarrass zinc of its arsenic, and the process of reducing zinc from a pure oxyd is very laborious and uncertain; how then can zinc free from arsenic be obtained? There is but one satisfactory answer to this question. Pure zinc might certainly be made from the oxyd by the manufacturing chemist, but at present the zinc labelled "pure" by those who sell chemicals is not to be relied upon, and the chemist must therefore test samples procured from the dealer in metals, until he finds a specimen of the pure zinc which is manufactured on a large scale, and is to be obtained in commerce.

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* Since this paper was written, Mr. Wharton, director of the Bethlehem Zinc Works has prepared a large quantity of chemically pure zinc for chemical use, in ingots of about ten pounds weight. See his advertisement in this Journal.—Eds.

Art. XLIV.—*The Gold of Nova Scotia;* by O. C. Marsh, A. B., of the Sheffield Scientific School, Yale College.

On the Atlantic coast of Nova Scotia is a belt of metamorphic rocks extending the whole length of the Province and varying in width from ten to fifty miles. It is mainly composed of clay slate and quartzite, but in some parts of the district these are replaced by mica slate, gneiss and granite. These strata have a general N.E. and S.W. course and are highly inclined. They have received but little attention from geologists and as no fossils have yet been found in them their exact age has been a matter of considerable uncertainty. Prof. J. W. Dawson, who from his study of this region is best qualified to express an opinion on this point, states that they are probably Lower Silurian, and possibly of the same age as the Potsdam sandstone.*

The general resemblance of these strata to the gold-bearing rocks in other parts of the world had occasionally been noticed, and various explorations for the precious metals had from time to time been made in their vicinity, but I cannot ascertain that gold was actually discovered in this Province earlier than March, 1860, although reports to that effect have been circulated. It was then accidentally found in Halifax county, about fifteen miles from the coast, in the bed of a small stream which empties into the Tangier river. Gold was soon after observed in the adjacent quartz veins also, and in a short time several hundred persons were attracted thither by the reports of the discovery and commenced explorations. The quantity of the gold obtained, however, was so small, that the excitement soon diminished, and but one company continued work for any length of time. In May of that year, the Provincial Secretary, Hon. Joseph Howe, accompanied by Prof. How of King's College, made an official visit to the locality, and on his return published a report which was very unfavorable to future explorations.

The discovery of gold in the Province, although in small quantity, naturally encouraged a further search, and in March of the present year it was again found, on the coast near Tangier harbor, in sufficient abundance to promise profitable employment for a great number of persons, and since that time a large amount has been obtained from that locality. Within the next three months gold was discovered in the same strata at various other places, the most important of which are Rawdon and Douglass, in Hants county; Gold river, near Chester; and Lawrencetown, a few miles east of Halifax. At the latter place there are indications of an extensive deposit of gold, and an association, organized in London, under the name of the "Nova Scotia Gold Mining Company," has recently purchased a tract of land there, and obtained permission from the government to work it for a term of years. In June last, gold was discovered in a bluff on the coast near Lunenburg, and shortly after the sands on the beach below were found to be unusually rich in this metal. It has also been found quite recently at Lake Thomas, about fifteen miles north of Halifax, and some valuable specimens obtained.

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* Supplement to Acadian Geology, page 53.

While in Nova Scotia a few weeks since I visited Tangier and Lunenburg, the most important of the above localities, and through the kindness of Mr. S. P. Fairbanks, the Provincial Inspector of Mines, I had an opportunity of examining the gold-bearing strata at these places and in their vicinity. I am also indebted to this gentleman for many interesting facts in regard to the discovery of the gold.

The Tangier mines are situated sixty-seven miles east of Halifax and about half a mile from the coast. Here the out-cropping rocks form a series of low hills, which are covered with a thick growth of spruce and hemlock. The strata which contain the gold consist of clay slate, traversed in various directions by veins of quartz, which is generally very compact. The cellular variety, discolored by oxyd of iron, so commonly found with the gold in California and Australia, appeared to be wanting at this locality. The strata, which are here very much disturbed, had been well exposed in many places by the recent explorations, but the nature of the surrounding country prevented any extensive examination of them. At one point they had a strike of S. 84° E., and a dip of 67° S.

The excavations at Tangier were carefully examined for fossils but without success, as the igneous action to which these rocks have been subjected has probably obliterated all traces of those they once contained. The recent discovery, however, of very perfect fossils, of many new species, near Saint John, New Brunswick, in clay slate which closely resembles this in structure, would seem to indicate that some organic remains may have been preserved in this formation.

The gold at Tangier occurs mainly in the quartz veins, which are in most cases less than a foot in width, but in one instance I noticed it in the argillite near its junction with the quartz. It is disseminated through the matrix in the usual manner,—frequently in isolated particles and masses, and where the quartz is white furnishes specimens of great beauty. One of the largest obtained was prized at three hundred dollars, which was but little above its intrinsic value. Gold has also been found in the soil, and in the bed of a small stream near the mines; but not in sufficient quantity to attract much attention.

The minerals noticed in association with the gold at this locality were mostly iron pyrites and mispickel. The former appeared to be quite abundant, and, suspecting it to be auriferous, I have examined a specimen and find it contains a considerable quantity of gold. The exact amount was not estimated, but it is sufficient to make its separation profitable if conducted with skill and economy. The mispickel at Tangier is frequently found underlying the gold in the quartz veins, and in some cases enclosing it. Chalcopyrite, magnetite, hematite, and galena, also occur in small quantities.

Among the specimens of gold obtained at Tangier I noticed three isolated crystals, which resembled in general appearance those brought from California. The largest of these was about one third of an inch in diameter. It was a rhombic dodecahedron with its edges slightly beveled, and although its faces were marked with delicate striæ several of them were unusually brilliant. The other two crystals were octahedrons, with dull and somewhat rounded faces. One of these was flattened and also much elongated. The smallest crystal was about two lines in length and quite perfect.

The mines at this locality are on the Government lands, and a 'claim,' thirty by thirty-three feet, is rented at twenty dollars per annum. At the time of my visit in August, about seven hundred men were working 'claims,' and a large amount of gold had been taken from the quartz veins, although in many cases at least one third of what they contained was lost by the rude and unsatisfactory methods employed in its extraction. Two crushing mills, however, were then nearly completed, which, although very unlike, were apparently well adapted to the end in view. One of them was very similar to the *arrastre*, a rude instrument used extensively in the silver mines of Mexico, and found to be very effective.* It consisted, essentially, of two large granite boulders, attached by short ropes to a horizontal beam, on either side of an upright shaft, around which they were drawn by a pair of horses. The quartz was put on a paved floor and kept wet, and was crushed by the boulders as they were dragged over it. The other mill was a small sized quartz-crusher of recent invention.

At Lunenburg, which is about seventy miles west of Halifax and one hundred and thirty from Tangier, the gold also occurs in quartz veins traversing the clay slate, which here forms a high bluff, but it is most abundant in the sands of the adjacent beach. Those who first commenced explorations at this place obtained large quantities of gold with very little labor, and their success soon attracted others from all parts of the Province. This locality is known in the neighborhood as "The Ovens," from some deep caverns which have been worn in the bluff by the action of the sea. It is this denuding power which has torn the gold from its bed and collected it on the beach. There is some reason to believe that a large amount of gold derived from the same source exists in the bottom of the harbor, as the sea-weed which is washed on shore has occasionally small particles of the precious metal attached to it. This point will probably soon be decided; as a "Dredging Company" has been formed, and in a short time will commence operations.

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* Ure's Dictionary of Arts, vol. iii, page 677, London, 1860.

The strata at this place are similar in appearance and structure to those at Tangier, and seem to have been equally disturbed. At one point near the shore where they were well exposed the strike was S. 80° W., and the dip about 75° N. Quartz veins pass through the slate in many directions, and are generally found to contain gold, especially those running north and south. Several dikes of basaltic trap were also observed, one of which was seven feet in width and appeared to be conformable to the strata. The auriferous sand on the shore rests on the edges of the upturned slate, which has here been worn out into 'pockets' of various sizes, well adapted to retain the gold as it is washed over them. After these cavities have been apparently exhausted, a large amount of fine gold can be obtained, for several feet beneath them, between the thin laminæ of the slate.

Nearly the same minerals which were noticed at Tangier also occur with the gold at this locality. The mispickel is more abundant, and is usually in very perfect octahedral crystals, some of which are twins and highly modified. The large amount of this substance in the sand on the beach, makes the gold washing somewhat difficult, and with the rude apparatus employed much of the fine dust is lost. Mercury has not yet been used in separating the gold either here or at the other localities.*

It is impossible to form any reliable estimate of the amount of gold obtained in Nova Scotia since its discovery there in March last, as in almost every instance the 'claims' have been worked by private individuals who were generally disinclined to give information in regard to their own success. Nor would the amount alone, if ascertained, be a fair criterion by which to judge the value of the gold fields, since they have in most cases been explored by those who have had no previous experience in searching for gold, and only the rudest methods have been employed in obtaining it. I was informed that gold to the value of $2400 had been taken from one 'claim' at Tangier, $1300 from another, and $480 from a third, although many other 'claims' had yielded little or nothing. I saw in Halifax ingots and specimens of Tangier gold which were valued at about $2000, and at Lunenburg at least $250 worth of fine dust which it was said had been washed from a single 'pocket' on the beach.

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* While at Lunenburg I was informed of a circumstance connected with the discovery of the gold which illustrates the utility of even a little scientific knowledge, and the need of its more general diffusion. Some years since a farmer, living in the neighboring town of Chester, thought he had discovered a valuable copper mine on his land, and at a great expense sunk a shaft about eighty feet in depth. Finding little copper to repay his labor, and having exhausted all his means, the work was finally abandoned. In his excavations he had cut through a large quartz vein richly stored with gold, which he had noticed, but supposed to be merely copper pyrites. The present owner works this copper mine for gold.

I have recently analyzed some specimens of gold which I obtained at Tangier and Lunenburg, and the results are given below. The Tangier specimen was taken from a quartz vein, and is very remarkable for its purity. I find it is surpassed in this respect by the gold from only one other locality, viz., Schabrowski, near Katharinenburg, in Siberia.* The Lunenburg gold was in small particles, washed from the sand on the shore. In preparing for the analyses the gold was boiled in chlorhydric acid, fused twice with borax and hammered, and its specific gravity taken. The quantity employed in each case was between one and two grammes, and the analyses were made according to the method used by Rose in his investigations on the gold of the Ural mountains.†

An analysis of the Tangier gold, specific gravity 18·95, gave,

Gold, - - - - - - -	98·13
Silver, - - - - - - -	1·76
Copper, - - - - - - -	·05
Iron, - - - - - - - -	*trace.*
	99·94

An analysis of Lunenburg gold, specific gravity 18·37, gave,

Gold, - - - - - - -	92·04
Silver, - - - - - - - -	7·76
Copper, - - - - - - -	·11
Iron, - - - - - - - -	*trace.*
	99·91

In some specimens of auriferous quartz from Lawrencetown, obtained of Mr. R. G. Fraser of Halifax, I found mispickel, iron pyrites, galena, and magnetite, associated with the gold in the same manner as at the other localities. In one instance a crystal of mispickel had a small particle of gold passing directly through its center. The specific gravity of the gold from this place was 18·60, which would indicate a degree of purity between that of the Tangier and Lunenburg specimens. The quantity obtained was not sufficient for satisfactory analyses.

Mr. Fraser informed me that some time since, in company with several others, he made explorations for gold on Sable island, and found a small quantity in the sand of which it is composed. As this island is more than one hundred miles from the coast, this discovery would appear to indicate that the gold-bearing strata of Nova Scotia extend for a considerable distance beneath the Atlantic ocean.

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* Dana's Mineralogy, Fourth ed., page 9.

† Reise nach dem Ural, page 406. Berlin, 1842.

There is another belt of metamorphic rocks in the northern part of this Province which resembles in many respects that on the Atlantic coast, although it probably belongs to a more recent formation. The Cobequid mountains are in this district, and are mainly composed of talcose and chloritic slates, penetrated by dikes of green-stone, sienite and granite. While passing this range in August last, in company with Mr. W. P. Ketcham of New York, I noticed a close resemblance between these rocks and the auriferous strata which I had just examined at Tangier and Lunenburg. The quartz veins were of similar size and appearance, and contained some of the same minerals which are there associated with the gold. I think it probable that these strata also will be found to contain this metal, although the hasty and imperfect examination, which we then were enabled to make, was not rewarded by its discovery.

A public geological survey of Nova Scotia is much needed, and a considerable part of it could be made with comparatively little labor; as in some parts of the Province the formations are so interesting that they early attracted the attention of scientific men, and have been very carefully studied. The districts, however, in which gold has been discovered, and in which it is likely to be found, have been only casually examined, and a systematic survey would make known their real value and prevent the recent discoveries from proving a misfortune, by impairing more important branches of industry. Now that the monopoly of the "General Mining Association," which has so long obstructed the development of the rich mineral resources of the Province, has been removed, it seems especially desirable that this survey should no longer be delayed. The revenue derived from the rent of 'claims' in the gold fields would probably be more than sufficient to carry on the work and could not well be devoted to a better purpose.

The great extent of metamorphic strata in Nova Scotia, so similar to the gold-bearing rocks in other countries, and the fact that gold has now been found at many widely separated points, would seem to indicate that a new and important source of mineral wealth will soon be added to this already favored Province.

Sheffield Laboratory, Yale College, Oct. 5th, 1861.

ART. XLV.—*Notice of a Meteorite which fell in Hindostan in 1857*; by J. LANG CASSELS, M.D., Professor of Chemistry, Cleveland Med. Coll., Ohio.

THIS meteorite fell near the small village of Parnallee in the extreme south part of Hindostan, and was obtained and sent to the Western Reserve College, Ohio, by the Rev. H. S. Taylor, a graduate of that Institution, at present connected with the Madura mission. Along with the stone he sent the following account of its fall, &c.

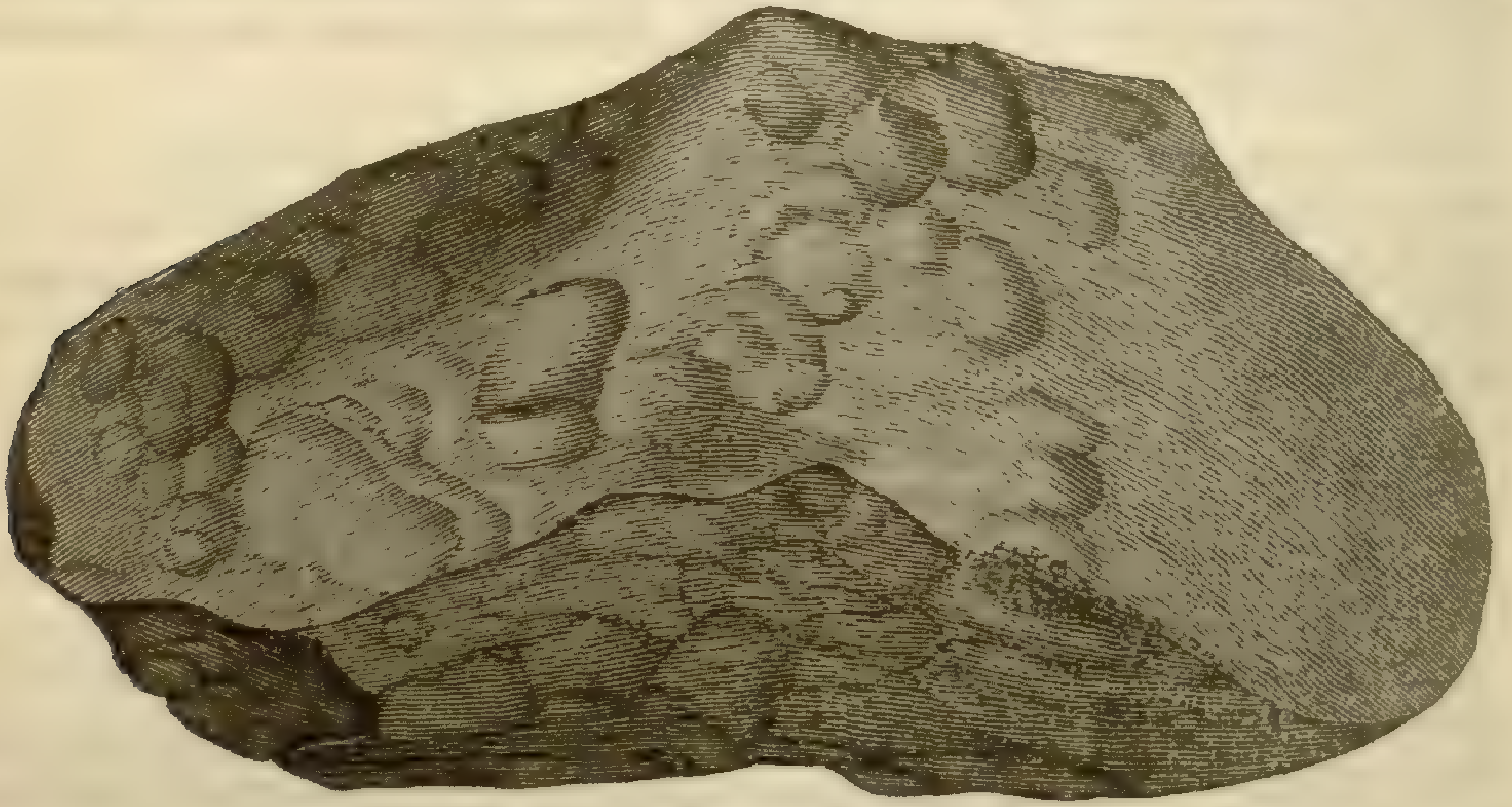
"Two meteoric stones fell on the 28th of February, 1857, from a clear sky, about noon, near the village of Parnallee, where some of our Christian people live. The smaller one weighs 37 pounds, and the larger is three or four times as heavy. The larger fell first, the smaller two or three seconds after, and some two or three miles south of the first one. The larger falling into tenacious and hard earth sunk into the ground but two feet and five inches. It came from the north, making an angle with the vertical of about fifteen degrees.

"The smaller one fell perpendicularly and sunk into the ground two feet eight inches. As no rain had fallen since they fell, I was able, on going there three days ago—April, 1857—to make sure their depth, to see just the impression they left when taken up, and to assure myself by enquiry and observation as to the stones having fallen there. Some children were picking cotton within a few rods of the first when it fell; and two women were standing near the place where the second fell. A cloud of dust was seen to be raised in each case, for the ground was very dry. Before night the larger stone was visited by crowds of persons from the neighboring villages, who commenced worshiping it as the image of their deity which had fallen from heaven."

The noise which these meteorites made while passing through the atmosphere is described as being terrific to all in the vicinity; and Mr. Taylor adds, that it was reported to have been heard some 15 or 20 miles from where they fell.

With much difficulty Mr. Taylor succeeded in obtaining both meteorites from the natives, who closely clung to them with great reverential attachment; but the Madrass government having learned of their fall, claimed them as a matter of right, and they accordingly were taken from him and placed in the Madrass Museum where the larger one still remains. Through the influence of some friends, Mr. Taylor, with commendable zeal and much perseverance, succeeded in regaining the smaller one, which he generously sent to the mineralogical cabinet of his alma mater in Hudson, Ohio.

This meteoric stone has all the appearances of this class of meteorites; it is coated with the usual black vitrified crust, and although angular in its general outlines, it is more or less rounded on these angles; it has numerous spots on its surface, varying in size from a line to an inch and a half in diameter, with a corresponding varying depth; some of them being a quarter of an inch deep—as is attempted to be shown in the accompanying figure. Internally it is a mottled grey color, having numerous circular spots of dull white. Throughout its recent surface are numerous brilliant specks of nickel, and distinct crystals of nickeliferous iron with a good deal of iron rust. Olivine and schreibersite can also be identified with a magnifier.



The meteorite is remarkable for the great amount of nickel it contains—nearly 17 per cent, while the iron is about 3 per cent. This metal is not uniformly distributed throughout the mass, as is plain even by an examination of the surface by a magnifier.

The magnet abstracts 21·151 per cent from the powdered stone. Color of the powder olive green. Sp. gr. 3·421—3·464.

An examination of the stone detected the presence of silica, lime, potassa, soda, oxyd of iron, sulphid of iron, oxyd of chromium, oxyd of manganese, iron, nickel, cobalt, copper, sulphur and phosphorus.

[A pisolitic structure is very evident in the stone, spherical masses of meteoric pyrites enclosing often a minute granule of white silicate, and surrounded with a coating of a blackish color and magnetic. A similar pisolitic structure has been noticed in other meteorites, as for example in that of Weston, Conn., (1807, Dec. 14). The mottled character of the fresh fracture, presenting large patches of gray and white contrasted with a darker ground, is strikingly similar in these two stones. Very unlike however is the distribution of the iron which in the Connecticut stone exists in nodules of some size, while in the Parnallee stone there is a remarkable absence of particles larger than a pin's head. The surface of the Parnallee stone under a file shows countless points of metallic reflection, the metallic nickel being almost in a spongy state resembling silver reduced from its chlorid by zinc. The mineralogical constituents of this stone are clearly brought out on a polished surface, which then presents a porphyritic appearance.*]

ART. XLVI.—*On an Improvement in the Lenticular Stereoscope;*

by Prof. E. EMERSON, Troy University.

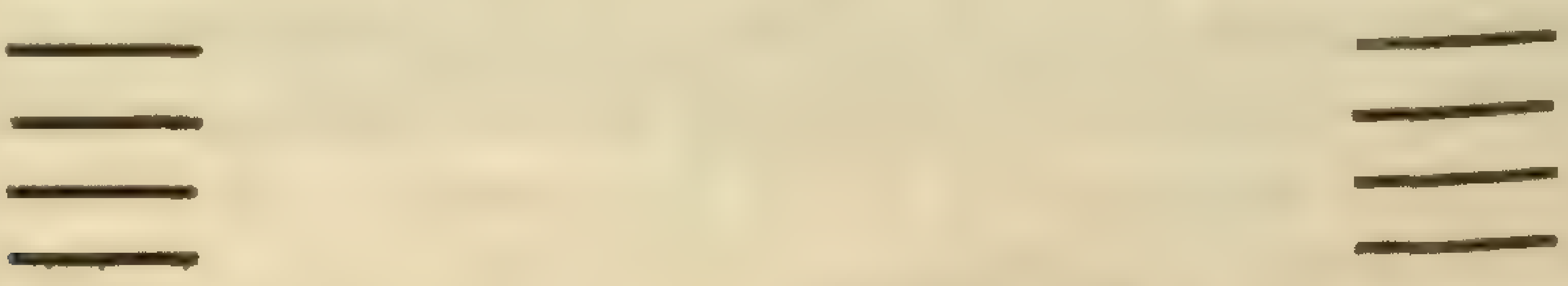
THE Lenticular Stereoscope, in its common form, has certain imperfections as an instrument which more or less detract from the pleasure which would otherwise attend its use. Nor are these imperfections of such a character as only to be recognized by adepts in its use; on the contrary, they are well known to almost every one who possesses one of these popular and instructive instruments. The causes of these imperfections are not so generally known. A common result of one fault is the difficulty which is experienced in endeavoring to unite the dissimilar right and left pictures; this is, indeed, sometimes quite impossible, and the observer, after a series of exercises, exceedingly straining to the organ of vision, gives over the effort in despair. Another imperfection is the inability of the instrument to exhibit a steroscopic view of pictures much larger than those ordinarily furnished by the dealers in Europe and America. Negatives including a much larger angle are readily obtained, but the positives taken from them are either reduced in copying, or the negatives are cut down to the size of the ordinary stereoscopic picture, when they are printed by contact; thus, oftentimes, some of the most beautiful details in the view are absolutely sacrificed.

* Prof. Casssel's notice of the Parnallee meteorite has been in our hands for some months. We have taken the liberty to add some details to the mineralogical description, having by the kindness of Prof. Young of Hudson, O., had the opportunity of inspecting the stone now in the Cabinet of the Western Reserve College. Understanding from Prof. J. L. Smith that he was engaged in an analysis of this meteorite, we at once suspended a similar analysis then in hand—believing that a chemist so much in the habit of conducting the chemical examination of meteors would do the subject fuller justice than is possible for one not constantly engaged in similar analyses—always difficult and unsatisfactory. Prof. Smith's results have not been received and we do not feel at liberty longer to withhold Prof. Cassels' paper, which has now a new interest in connection with the remarks of Mr. Haidinger, (see *Meteorology*.)

In a private letter Dir. Haidinger says: "As to the structure of the Parnallee meteor I have compared it with those in our Imperial Cabinet and find that among them all it has the closest resemblance to the meteorite which Piddington of Calcutta discovered among a lot of rocks from Assam in 1846." (See this vol., p. 143.)—S.

We propose in the present article—1st, to account for these imperfections in the stereoscope, and 2d, to describe a simple modification of the instrument, which will remove them completely.

The difficulty experienced in uniting the right and left pictures of certain stereoscopic slides may be occasioned either by the lenses being improperly mounted; or the pictures being arranged on the slides at improper distances from each other; or because the eyes of one observer are naturally wider apart than those of another, and, therefore, the same instrument fails to afford an equally good view to each. Let us examine, briefly, each of these cases. It sometimes happens that the lenses are so adjusted or cut as to occupy improper positions; as a consequence one of the pictures is thrown a little *higher* than the other. If this defect operated only in a horizontal direction it would not be so serious, as, within certain limits, the eyes can accommodate themselves to a horizontal strain; which accounts for our ability to see certain views after a considerable effort, which at first gave us more or less difficulty. But when the discrepancy is one measured on a perpendicular line, it is much more serious; the eyes are not accustomed to move in this direction independently of each other; so that if the imperfect mounting of the lenses causes a variation on this line of from one to two tenths of an inch it will be almost impossible for the observer to unite the views. An easy method of testing an instrument for this fault is to draw upon a piece of white paper the size of a stereoscopic slide, two series of short lines, each series being 2·6 inches from the other, and the lines in each series being drawn at corresponding distances from each other, thus:



Upon placing this in the stereoscope, and looking at it, if the lines unite instantly with no variation, it proves the absence of this fault; but if, on the contrary, we obtain as a resultant a view of more than four lines, thus:

we may be certain that the lenses are not properly mounted, and will give in usage little or no satisfaction. The instrument, however, may be perfect as to its lenses, and a difficulty still be experienced as to certain pictures or by certain persons. When the imperfection consists of a want of proper relation between the lenses and the slide, it usually results from the complementary pictures being mounted at too great a distance from each other for the power of the fixed lenses to unite them; but the same result may be the effect of an individual peculiarity in the observer, the eyes being naturally wider apart than is common. The variation in the mounting of stereoscopic pictures is very great. In carefully measuring a lot of nearly three hundred different views, on glass and paper, French, English and American, the actual variation was over an inch between the extremes; some being mounted only 2¼ inches apart, and not a few of them over 3¼ inches from each other. Moreover, as might be expected from the variation in practice, there is considerable difference in opinion, among those who have endeavored to settle this point, as to what should be the standard distance between the right and left pictures. Some say, 'the same distance the eyes are separated,' others '2·6 inches,' and a recent writer, Mr. Shadbolt, in the British Journal of Photography, says—"There being two variable quantities involved, viz: the width between the eyes of different observers, and the lateral displacement of rays by the prisms used, it is necessary to obtain something like an approach to an average of the amount of these inconstant quantities, and after careful consideration of the matter, we fixed it at 2·75 inches." The same writer, however, finds the distance required by Smith and Beck's achromatic stereoscopes very constant at 2·8 inches. It does not seem that there is a clear apprehension of the truth in regard to this point; which is that the distance the pictures should be mounted apart will depend entirely upon the deflecting power of the lenses employed to unite them. And as there does not appear to be any standard for this power of the lenses, there is, consequently, none to regulate the distance the right and left hand pictures should be separated. It is easy to see, therefore, that a stereoscope which will enable us to see, equally well, different views which vary more than an inch in the mounting is a desideratum.

The lenses of the ordinary stereoscope are so mounted that the eyes of the same observer always look through the same portion of each lens, which of course always causes the same amount of deflection, and answer perfectly to unite pictures which are separated from each other a certain, uniform distance. In order to measure this distance for any particular instrument take ten or fifteen pieces of white paper or card-board, the size of a stereoscopic slide, and upon the first, draw two lines one quarter of an inch in length, perpendicular to the lower side of the card, and parallel to each other, thus:

increase the distance between the lines upon each successive card one-tenth of an inch until the lines upon the last card are three and one-half inches apart; now look at each of these in the stereoscope with a rather hasty glance, and it will be easy to determine at what distance the lines should be apart to coalesce perfectly and instantly. This will be the distance the centres of the right and left pictures should be separated *for that instrument.*

But further, the ordinary lenticular stereoscope does not permit the view of a picture measuring over three inches from one side to the other; i. e., it affords a view of nine square inches as a maximum.* Sir D. Brewster, in his work on the Stereoscope, pp. 162, 163, declares that—"the *size* of the pictures is determined," "that nothing can be gained by using larger pictures," &c., &c. This is true with regard to the ordinary form of the stereoscope. But this renders it none the less a very desirable thing to be able to see larger pictures: and by larger pictures we do not mean the same pictures magnified, for then, indeed, nothing would be gained, but pictures including a larger angle, and affording a view of more than twenty square inches as a maximum.

Before proceeding to a description of an improvement which accomplishes this result, it will be necessary briefly to remind the reader that the lenses employed in the stereoscope while they are constant as to their focal length, vary exceedingly as to their power of deflecting a ray, increasing it regularly as we proceed from the centre of the lens to its thin edge; so that pictures which can be easily united when seen through the thicker or central portion of the lenses, require to be separated more and more from each other as we separate the glasses, and thus force the eyes to use a more highly deflecting portion of the lenses. The modification we propose in order to give the instrument a general character, and adapt it to all sorts of views is an adjustment by which the lenses are rendered movable in a horizontal direction, so that they can be readily approached near to each other until the edges touch, or separated from each other as far as the distance between the eyes will allow; i. e. the distance traveled by each lens will be measured by the distance between the thickest and thinnest portions of that lens, which will be a little over an inch. To operate properly the lenses should move simultaneously, at uniform rates and in opposite directions. As the right lens moves towards the right, the left lens moves towards the left, and vice versa. By this means the lenses are made equivalent to prisms with variable angles. An easy method of accomplishing this is shown in the following diagram exhibiting the *under side* of the lenses and their mechanical attachment.

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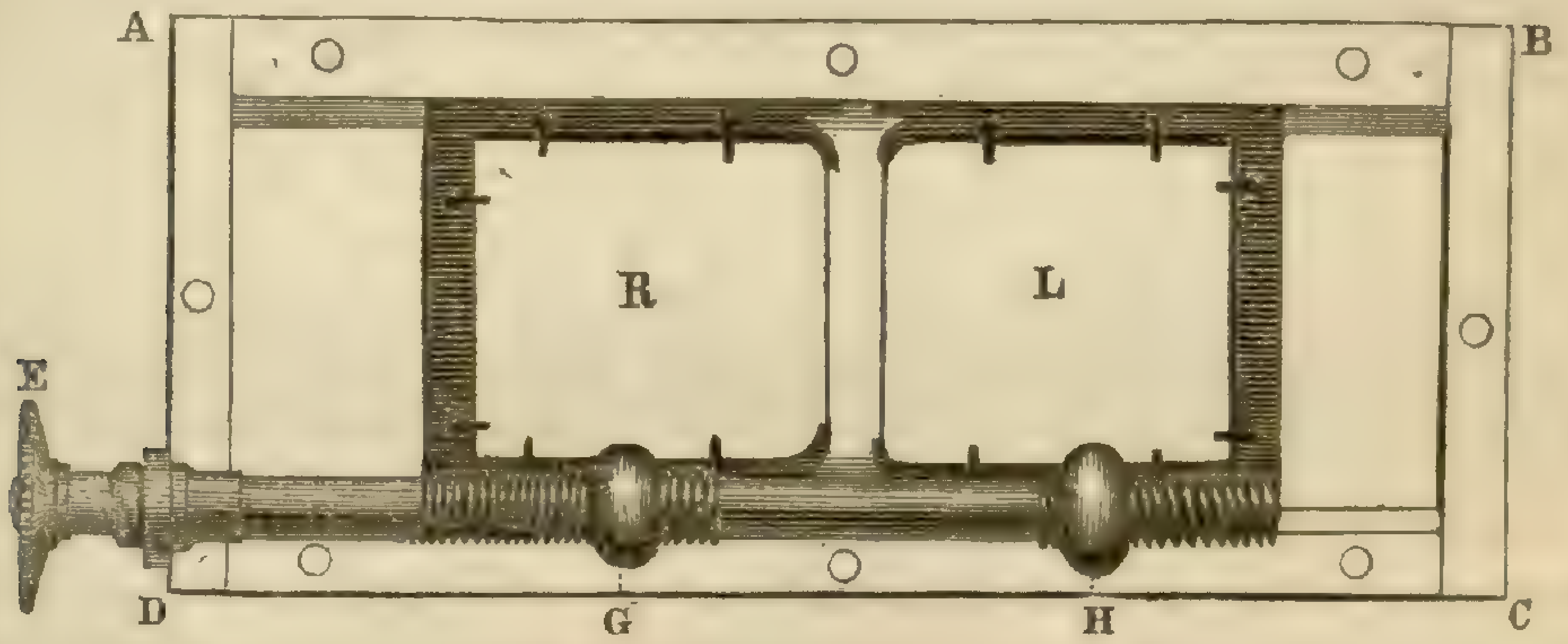
* The Reflecting Stereoscope of Wheatstone will exhibit larger pictures, but it is not adapted to popular use.

This apparatus takes the place of the lenses in the various forms of the lenticular stereoscope. We have adjusted our own to a common hand-stereoscope merely flanging the sides so as to afford a view of glass and paper slides measuring ten inches in length by four inches in width.



A B C D, light brass frame in which the lenses slide.
R, right-hand lens. L, left-hand lens.
G, nut attached to the setting of the right lens through which a right-hand screw turns.
H, nut attached to the setting of the left lens, through which a left-hand screw turns.
E, Milled head on the end of the rod upon which is cut the right-hand screw for the nut G and also the left-hand screw for the nut H.

By turning the milled head E the lenses either advance towards each other or recede from each other as may be desired.

By means of this instrument, whatever the distance between the eyes, pictures may be easily and perfectly united which are mounted at any distance apart between two, and four and a half, inches from centre to centre; and this, too, with the ordinary stereoscopic lenses; if achromatic lenses are used, it is quite practicable to unite pictures which measure five inches from centre to centre, affording a view filling twenty square inches, which is considerably more than twice as large as the ordinary slides.

Troy University, Oct., 1861.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

PHYSICS.—

1. *Photographs of Spectra exhibited to the Chemical Section of the British Association;* by Prof. W. A. MILLER (Chairman). He remarked: —The apparatus by which the spectra may be photographed consists of an ordinary camera obscura attached to the end of a long wooden tube, which opens into a cylindrical box, within which is a prism glass, or a hollow prism filled with bisulphid of carbon. If the prism be so adjusted as to throw the solar rays, reflected from a heliostat, upon the screen of the camera, and the wires which transmit the sparks from a Ruhmkorff coil are placed in front of the uncovered portion of the slit, the two spectra are simultaneously impressed. The solar beam is easily intercepted at the proper time by means of a small screen, and the electric spectrum is allowed to continue its action for two or three, or six minutes, as may be necessary. He did not find that anything was gained in distinctness by interposing a lens of short focus between the slit and the wire which supplied the sparks, with the view of rendering the rays of the electric light parallel like those of the sun, owing to the absorbent action of the glass weakening the photographic effect; and the flickering motion of the sparks being magnified by the lens, rendered the lines less distinct than when the lens was not used. Although with each of the metals (including platinum, gold, silver, copper, zinc, aluminium, magnesium, iron), when the spark was taken in air, he obtained decided photographs, it appeared that in each case the impressed spectrum was very nearly the same, proving that few of the lines produced were those which were characteristic of the metal. The peculiar lines of the metal seemed chiefly to be confined to the visible portion of the spectrum, and these had little or no photographic power. This was singularly exemplified by repeating the experiment upon the same metal in air, and in a continuous current of pure hydrogen. Iron, for example, gave, in hydrogen, a spectrum in which a bright orange and a strong green band were visible, besides a few faint lines in the blue part of the spectrum. Although the light produced by the action of the coil was allowed to fall for ten minutes upon a sensitive collodion surface, scarcely a trace of any action was procured; whilst, in five minutes, in the air, a powerful impression of numerous bands was obtained. It is remarked by Mr. Talbot that, in the spectra of colored flames, the nature of the acid did not influence the position of the bright lines of the spectrum, which he found was dependent upon the metal employed, and this remark had been confirmed by all subsequent observers. But the case was very different in the absorptive bands produced by the vapors of colored bodies,—there the nature of both constituents of the compound was essentially connected with the production of absorptive bands. Chlorine, combined with hydrogen, gave no bands by absorption in any moderate thickness. Chlorous acid and peroxyd of chlorine both produced the same set of bands, while hypochlorous acid, although a strongly colored vapor and containing the same elements, oxygen and chlorine, produced no absorptive bands.

Again, the brownish red vapor of perchlorid of iron produced no absorptive bands; but when converted into vapor in a flame this gave out bands independent of the form in which it occurred combined. These anomalies appeared to admit of an easy explanation on the supposition that, in any case, the compound is decomposed in flame, either simply by the high temperature, just as water is, as shown by Grove, or, in all other cases of the production of bright lines by the introduction of a metallic salt into a flame of burning bodies (as shown by Deville). In the voltaic pile the decomposition must of necessity take place by electric action. The compound gases, protoxyd and binoxyd of nitrogen, gave, when electrified, the same series of bright bands (as Plücker had shown) which their constituents when combined furnish. Aqueous vapor always gives the bright lines due to hydrogen and hydrochloric acid, the mixed system of lines, which could not be produced by hydrogen and chlorine. The reducing influence of the hydrogen and other combustible constituents of the burning body would decompose the salt, liberating the metal, which would immediately become oxydized or carried off in the ascending current. There was obviously a marked difference between the effect of intense ignition upon most of the metallic and the non-metallic bodies. The observations of Plücker upon the spectra of iodine, bromine and chlorine show that they give, when ignited, a very different series of bands to those which they furnished by absorption, as Dr. Gladstone had already pointed out; but it was interesting to remark that, in the case of hydrogen, which, chemically, was so similar to a metal, we have a comparatively simple spectrum, in which the three principal bright lines correspond to Fraunhofer's dark lines, C, F, and G. It was, however, to be specially noted that the hydrogen occasioned no perceptible absorptive bands at ordinary temperatures in such thickness as we could command in our experiments, and the vapor of boiling mercury was also destitute of any absorptive action, although when ignited by the electric spark it gave a characteristic and brilliant series of dark bands. The following experiment suggested itself as a direct test of Kirchhoff's theory. Two gas-burners, into which were introduced chlorid of sodium on the wick of the spirit lamp, were placed so as to illuminate equally the opposite sides of a sheet of paper partially greased. The rays of the electric light screened from the photometric surface, suitably protected, were made to traverse one of the flames. If the yellow rays of light were absorbed by the sodium flame, the light emitted laterally by the flame should be sensibly increased. The experiment, however, failed to indicate any such increase in the brilliancy of the flame, possibly because the eye was not sufficiently sensitive to detect the slight difference which was to be expected.—*Athenæum*, Sept. 14, 1861.

CHEMISTRY.—

2. *On Cæsium and Rubidium.*—BUNSEN has communicated a preliminary notice of the two new metals discovered by Kirchhoff and himself by means of the spectral analysis. Both of these metals exhibit in their compounds an extraordinary resemblance to potassium, and cannot be distinguished from it either by reagents or by the blowpipe. The first of the new metals is named Rubidium from *Rubidus*, dark red, referring to two very remarkable spectral lines, which lie beyond Fraunhofer's line A,

and consequently occur in a portion of the spectrum which can only be rendered visible by unusual methods. The authors have found the metal in the greatest quantity in the lepidolites; that of Rozena, in Moravia, contains about $\frac{2}{1000}$ of its weight of oxyd of rubidium—the Saxon lepidolite appears to be still richer. Traces of it are found in almost all mineral waters, but it does not appear to be always present in the potash compounds of commerce. The compounds of Rubidium are most easily obtained pure from lepidolite. The rubidium is contained with small portions of Cæsium in the precipitate of chlorplatinate of potassium obtained from the mineral. The chlorplatinate of potassium requires nineteen times, the chlorplatinate of rubidium one hundred and fifty-eight times its weight of boiling water for solution. The precipitate is to be boiled repeatedly with very little water, and the solution each time simply poured off from the precipitate, which exhibits in the spectral apparatus, after a few boilings, two new blue lines which lie close to the blue calcium line, which the authors have not represented in their first plate of the spectrum, because it is one of the weaker lines; by further treatment with water, the two red lines beyond *A*, soon appear, together with several others which are less characteristic. The chlorid of rubidium is easily obtained from the precipitate by reduction with hydrogen and extraction with water. In this manner the authors obtain from 150 kilogrammes of lepidolite, about two ounces of nearly pure rubidium salt. By repeating the process of precipitatiou with chlorid of platinum and boiling several times, the last traces of potassium may be removed. To obtain rubidium free from cæsium, the salt must be converted into carbonate and repeatedly extracted with alcohol, which dissolves the carbonate of cæsium.

Rubidium forms with mercury, with the aid of the Voltaic circuit, an amalgam of silver-white color and crystalline structure. This amalgam quickly oxydizes in the air, decomposes water in the cold and is electro-positive to potassium. The equivalent of rubidium is 85·36 and its symbol Rb.

The hydrate $RbO, HO + aq$ is soluble in almost all proportions in water and alcohol; gives off its water of crystallization by heating, leaving RbO, HO which on farther heating, melts but does not lose its atom of water, is caustic like potash, dissolves in water with strong evolution of heat, and greedily attracts water and carbonic acid from the air.

The carbonate RbO, CO_2 forms indistinct strongly alkaline crystals, insoluble in alcohol, and leaving on heating $RbO, CO_2 + aq$ as a sandy powder, which melts easily, deliquesces and absorbs an additional atom of carbonic acid. The bi-carbonate $RbO, CO_2 + HO, CO_2$ forms prismatic crystals permanent in the air, with a faint alkaline reaction, and a cooling, not caustic taste. The nitrate RbO, NO_5 is not rhombic like saltpetre but crystallizes in dihexagonal prisms, with less distinctly formed twelve sided pyramids. The sulphate forms large hard anhydrous crystals, having a glassy lustre and permanent in the air: they are isomorphous with sulphate of potash. This sulphate gives with sulphate of alumina an alum crystallizing in hard glassy octahedra, and with sulphate of cobalt a beautiful double salt isomorphous with $KO, SO_3 + CoO, SO_3 + 6HO$. The chlorid $RbCl$ is anhydrous, permanent in air, crystallizing with difficulty in cubes, easily fusible, and readily and completely volatile on the platinum wire. The chlorplatinate $RbCl, PtCl_2$ is a bright yellow, anhydrous, sandy powder, consisting of microscopic regular octahedra, which can only be distinguished from the chlorplatinate of potassium by its lesser solubility.

The second of the new elements, the authors term Cæsium, from *Cæsius*, sky blue; it gives a beautiful and highly characteristic spectral line, lying near the strontium-line *Srδ*. It appears to constantly accompany rubidium, but occurs for the most part in small quantity. Ten kilogrammes of the Dürkheimer water contain not quite two milligrammes of chlorid of cæsium. The authors obtained it from this water. The potassium, rubidium and cæsium are precipitated together by chlorid of platinum, and the double chlorids of potassium and rubidium, separated as much as possible by boiling with water; the cæsium may then be extracted by converting the mixed chlorids into carbonates, and dissolving out the carbonate of cæsium with absolute alcohol. To remove the last traces of potassium and rubidium, about $\frac{4}{5}$ths of the carbonate are rendered caustic by baryta water, the mass evaporated in a silver dish, and the caustic oxyd of cæsium dissolved out with absolute alcohol, which leaves the carbonates of potassium and rubidium. This operation must be repeated till the spectral analysis exhibits at most a very faint reaction for potassium and rubidium.

The amalgam of cæsium decomposes water in the cold and oxydizes in the air: it is electro-positive to both potassium and rubidium, and is therefore the most electro-positive of all known elements. The equivalent of cæsium is 123·4 and its symbol Cs.

The hydrate $CsO, HO+aq$ is indistinctly crystallized, deliquescent and extremely caustic; it loses an atom of water by ignition, evaporates completely when heated in the flame and is easily soluble in alcohol. The carbonate forms indistinct crystals, soluble in five times their weight of absolute alcohol, deliquescent and very caustic. The bicarbonate forms permanent, glassy prismatic crystals. The nitrate is anhydrous and isomorphous with nitrate of rubidium. The sulphate is also anhydrous and permanent in air; it forms a well crystallized alum and double sulphates isomorphous with KO, SO_3+MgO, SO_3+6HO. The chlorid crystallizes in cubes and is deliquescent in the air; it is slightly volatile and easily becomes somewhat basic, when heated in air. The chlorplatinate forms bright yellow microscopic regular octahedra, and is the least soluble of the three alkaline double chlorids.—*Ann. der Chemie und Pharmacie*, cxix, 107.

3. *Probable existence of a new Element.*—Crookes has proposed the name Thalium for what appears to be a new element existing in certain seleniferous and telluriferous deposits produced in the manufacture of sulphuric acid. The new element appears to belong to the oxygen and sulphur group, and produces in the spectrum an extremely pure and vivid green line comparable in intensity with the sodium line D. Thalium (θαλλος) was not detected in seleniferous and telluriferous minerals hitherto examined, but two or three specimens of native sulphur, among others those from Lipari, contain a small quantity, so that the sulphur of this locality may prove an available source of the substance in question. Thallium appears to be a dense brown powder characterized in a very re-

markable manner by the spectral line referred to. Its physical and chemical properties are not yet described.—*Chemical News, May,* 1861.

[The name Thalium is preoccupied, having been proposed by Dr. D. D. Owen (this Journal, [2], xiii, 420, 1852) for a supposed new metal found by him in *Thalite,* a greenish hydrous silicate from the north shore of Lake Superior.—Eds.]

4. *Reduction of Sulphuric Acid by Nascent Hydrogen.*—Kolbe has observed that sulphydric acid gas is always set free in perceptible quantity by the action of concentrated sulphuric acid upon metallic zinc. When the sulphuric acid is previously diluted with twice its volume of water perfectly pure hydrogen is obtained; the addition of concentrated acid immediately produced the odor of sulphydric acid. The hotter the liquid and the stronger the acid, the greater will be the quantity of sulphydric acid produced. Kolbe remarks that this fact is important in judicial investigations, since if concentrated sulphuric acid be used in Marsh's apparatus a part or perhaps the whole of the arsenic present might be converted into sulphid and thus escape detection.—*Zeitschrift für Chemie,* iv, *Jahrgang,* 419.

5. *On certain Organic Acids.*—Malic acid by the action of heat yields, as is well known, fumaric and maleic acids. Perkin and Duppa and Kekulè have shown that malic and tartaric acids may be prepared from succinic acid, and Kekulè now finds that fumaric and maleic acids may be readily converted into succinic acid or a derivative of this. When fumaric acid is heated for a few minutes in a water-bath with bromine and water, the bromine disappears, and on cooling white crystals of bibromosuccinic acid separate. The reaction is represented by the equation

$$C_8H_4O_8+2Br=C_8H_4Br_2O_8,$$

and consists not in a substitution but in a simple *addition* of bromine.

By the action of nascent hydrogen upon fumaric acid, succinic acid may be directly produced, a simple addition of H_2 taking place. Kekulè calls attention to the fact that this made of formation by *addition* has hitherto been unobserved in the case of the organic acids, and only rarely in the case of other organic bodies.

Maleic acid in like manner forms bibromosuccinic acid by the action of bromine, but another acid is formed at the same time which has not yet been studied. Maleic acid also combines with nascent hydrogen to form succinic acid. Iodhydric and bromhydric acids convert maleic into fumaric acid. Kekulè points out the following interesting parallels.

Succinic acid, $C_8H_6O_8-H_2=C_8H_4O_8$, Fumaric acid.
Propyl alcohol, $C_6H_8O_2-H_2=C_6H_6O_2$, Allyl-alcohol.
Propyl aldehyd, $C_6H_6O_2-H_2=C_6H_4O_2$, Acrolein.
Propionic acid, $C_5H_6O_4-H_2=C_6H_4O_4$, Acrylic acid.
Stearic acid, $C_{36}H_{36}O_4-H_2=C_{36}H_{34}O_4$, Oleic acid.
Marsh gas, $C_4H_6-H_2=C_4H_4$, Olefiant gas.
Alcohol, $C_4H_6O_2-2HO=C_4H_4$, " "
Malic acid, $C_8H_6O_{10}-2HO=C_8H_4O_8$, Fumaric acid.
Ethylene, $C_4H_4+2Br=C_4H_4.Br_2$, Bromid of ethylene.
Fumaric acid, $C_8H_4O_8+2Br=C_8H_4Br_2O_8$, Bibromsuccinic acid.
Ethylene, $C_4H_4+BrH=C_4H_5Br$, Bromid of ethyl.
Fumaric acid, $C_8H_4O_8+BrH=C_8H_5BrO_8$, Monobromsuccinic acid.

$$\left\{\begin{array}{ll}\text{Ethylene,} & C_4H_4 + H_2 = C_4H_6, \text{ Marsh gas.}\\ \text{Fumaric acid,} & C_8H_4O_8 + H_2 = C_8H_6O_8, \text{ Succinic acid.}\end{array}\right.$$

$$\left\{\begin{array}{ll}\text{Ethylene,} & C_4H_4 + 2HO_2 = C_4H_6O_4, \text{ Glycol.}\\ \text{Fumaric acid,} & C_8H_4O_8 + 2HO_2 = C_8H_6O_{12}, \text{ Tartaric acid.}\end{array}\right.$$

The analogy in these reactions cannot be doubted; the difference in properties and functions is easily explained by the fact that some of the substances contain oxygen, others none. More interesting and suggestive relations have seldom been brought forward.—*Ann. der Chemie und Pharmacie*, i, Supplement, Band 1, p. 129.

6. *Mathematical Theory of Homologous Series.*—Bacologlo has given an interesting mathematical development of the theory of homologous series considering the properties of bodies as functions of their constitution. Consider $C\alpha H\beta O\gamma$ as representing a single term of such a series; the properties of this body are functions of the qualitative and quantitative character of the forces which determine the equilibrium of the compound. These forces reside in the elements C, H, O and are more or less active according to the number of equivalents of each. If U be any property of the body $C\alpha H\beta O\gamma$, F an arbitrary function, a, b, and c the molecular forces or states of the elements C, H, O and α, β, γ the number of equivalents of each, we shall have:—

$$U = F(a, \alpha;\ b, \beta;\ c, \gamma)$$

For a second body $Ca{+}\Delta\alpha\, Hb{+}\Delta\beta\, O\gamma$ differing from the first only in the number of equivalents of C and H, containing therefore $\Delta\alpha$ carbon and $\Delta\beta$ hydrogen more or less, the corresponding property will have another value U_1 and we shall have

$$U_1 = F(a, \alpha + \Delta\alpha;\ b, \beta + \Delta\beta;\ c, \gamma)$$

It remains to determine the connection between U and U_1. Since a, b, c and γ are constants we may obviously write

$$U = F(\alpha, \beta)$$
$$U_1 = F(\alpha + \Delta\alpha, \beta + \Delta\beta)$$

whence by Taylor's theorem,

$$F(\alpha + \Delta\alpha, \beta + \Delta\beta) = F(\alpha, \beta) + \Delta\alpha \frac{d}{d\alpha} F(\alpha, \beta) + \Delta\beta \frac{d}{d\beta} F(\alpha, \beta) +$$
$$\frac{\Delta\alpha^2}{2} \frac{d^2}{d\alpha^2} F(\alpha, \beta) + \Delta\alpha\Delta\beta \frac{d^2}{d\alpha d\beta} F(\alpha, \beta) + \frac{\Delta\beta^2}{2} \frac{d^2}{d\beta^2} F(\alpha, \beta) + \&c.$$

Or,

$$(3.)\quad U_1 = U + \Delta\alpha \frac{dU}{d\alpha} + \Delta\beta \frac{dU}{d\beta} + \frac{\Delta\alpha^2}{2} \frac{d^2U}{d\alpha^2} + \Delta\alpha\Delta\beta \frac{d^2U}{d\alpha d\beta} + \frac{\Delta\beta^2}{2} \frac{d^2U}{d\beta^2} + \&c.$$

In homologous series $\Delta\alpha = \Delta\beta$ and we may write Δ for the increase in the number of equivalents of carbon and hydrogen, hence (3) becomes

$$(4.)\quad U_1 = U + \Delta \left\{ \frac{dU}{d\alpha} + \frac{dU}{d\beta} \right\} + \frac{\Delta^2}{2} \left\{ \frac{d^2U}{d\alpha^2} + 2\frac{d^2U}{d\alpha d\beta} + \frac{d^2U}{d\beta^2} \right\} + \&c.$$

whence for D the difference in the properties of the two bodies,

$$(5.)\quad D = U_1 - U = \Delta \left\{ \frac{dU}{d\alpha} + \frac{dU}{d\beta} \right\} + \frac{\Delta^2}{2} \left\{ \frac{d^2U}{d\alpha^2} + 2\frac{d^2U}{d\alpha d\beta} + \frac{d^2U}{d\beta^2} \right\} + \&c.$$

The first differential co-efficients $\frac{dU}{d\alpha}$, $\frac{dU}{d\beta}$, express the rate at which the property U varies with the increase of α and β; they express the quantity of the variation and may therefore be considered as constant. The second and higher differential coefficients consequently vanish and we have

$$D=U_1-U=\Delta\left\{\frac{dU}{d\alpha}+\frac{dU}{d\beta}\right\} \qquad (6.)$$

$$\text{and if } k \text{ be a constant}=\frac{dU}{d\alpha}+\frac{dU}{d\beta} \qquad (7.)$$

$$D=U_1-U=k\Delta \qquad (8.)$$

from which it follows that the difference D of the properties U_1 and U of two homologous bodies is proportional to the difference of their chemical constitution.

The complete law of the variation of the properties of homologous bodies is given by equation (5) and if we write S for the sum of the terms containing higher powers of Δ we shall have

$$D=U_1-U=k\Delta+S.$$

from which it follows that strictly speaking the difference of properties is not directly proportional to the difference of chemical constitution but is more nearly proportional the smaller the influence of the term S. This explains small deviations from the exact law in the case of boiling points, &c. The factor k will in general not be identical in the case of different homologous series. Thus in the series

$$C_4H_4Br_2$$
$$C_6H_6Br_2$$
$$C_8H_8Br_2$$

the property V is a function of the molecular states a, b, c and the numbers α, β, γ of the elements C, H, Br

$$V=\Phi(a,\alpha;\ b,\beta;\ c,\gamma)$$

the function Φ being different from F because the molecular force e differs from the molecular force c. Hence we shall have

$$D'=V_1-V=\Delta\left\{\frac{dV}{d\alpha}+\frac{dV}{d\beta}\right\}+S'$$

$$\text{or} \quad (10) \qquad D'=V_1-V=k'\Delta+S'$$

so that in this series also the difference in properties will be nearly proportional to the difference in constitution but will not be identical with the difference in the case of the first series, the modulus k' being different from k. This explains the variation in uniformity exhibited in Kopp's laws of serial differences. The author proposes to continue his investigations in the same direction. They amount, as it seems to the reviewer, to a somewhat elaborate though easy mathematical proof of that which can be shown to be necessarily true by much simpler reasoning.—*Journal für prakt. Chemie*, lxxxiii, p. 494.

7. *On the Conversion of Monobromid of Ethylene into Acetylene.*—Sawitch finds that the bromid $C_4H_3Br.Br_2$ when decomposed by an alcoholic solution of potash, gives bibromid of ethylene, which is always accompanied by a small quantity of other substances. When the vapors evolved during the reaction are conducted into an ammoniacal solution of suboxyd of copper, a dark red flocky precipitate is produced which,

on drying, detonates violently when heated or struck. This substance is identical with the red compound which acetylene forms with copper, whence it appears that acetylene is a product of the decomposition of bromid of ethylene, by an alcoholic solution of potash. It appears probable that a portion of the monobromid of Ethylene which occurs in this reaction loses an equivalent of bromid of hydrogen, and is converted into acetylene. The following equations will explain this reaction:

$$C_4H_4Br_2+KO, HO=C_4H_3Br+KBr+2HO$$
$$C_4H_3Br+KO, HO=C_4H_2+KBr+2HO$$

When monobromid of ethylene is heated with amylate of sodium, bromid of sodium, amylic alcohol, and acetylene are produced. The equation is

$$C_4H_3Br+\left.\begin{matrix}C_{10}H_{11}\\ Na\end{matrix}\right\}O_2=\left.\begin{matrix}C_{10}H_{11}\\ H\end{matrix}\right\}O_2+NaBr+C_4H_2$$

Sawitch has since found that the same reaction serves to form the homologues of acetylene—thus monobromid of propylene by the action of ethylate of sodium produces a new hydrocarbon, which the author terms allylene, and which has the formula C_6H_4. Allylene forms with copper solutions, a voluminous flocky yellow precipitate, which on heating burns quickly with a reddish flame; bromine decomposes it with effervescence and production of red flames; with concentrated acids it gives off gas even in the cold. Allylene is colorless; has a strong and disagreeable smell; burns with a bright and very smoky flame and precipitates solutions of silver and nitrate of suboxyd of mercury. Allylene combines with bromine to form a clear and colorless liquid which is probably $C_6H_4Br_2$. The author promises a more extended investigation.—*Journal für prakt. Chemie*, lxxxiii, p. 240 and *Comptes Rendus*, lii, 157 and 399.

8. *On the Reproduction of certain Crystalline Minerals.*—H. SAINT CLAIRE DEVILLE has succeeded in preparing a number of crystalline metallic oxyds by the action of a current of chlorhydric acid gas upon metallic oxyds heated in a platinum boat contained in a tube of porcelain. The temperature usually employed was that of melting copper. A volatile chlorid is at first produced and this is then decomposed by the vapor of water formed in the reaction, the temperatures of formation and decomposition being probably different. In this manner the author obtains stannic acid, SnO_2, in beautiful crystals of the same form as the native oxyd. Titanic acid forms brilliant crystals of a bluish tint like anatase. A mixture of chlorhydric acid gas and a reducing agent (hydrogen?) gave small crystals of a new saline oxyd having the formula TiO_2, Ti_2O_3 or Ti_3O_5, which is perhaps the true formula of anatase. Crystallized rutile is also formed by heating together to redness titanic acid and protoxyd of tin and then heating this mass with a silica to a cherry red heat. The crystals contain a small quantity of stannic acid. Deville finds vanadium in many rutiles; that of St. Yrieux is one of the richest ores of this metal. The author has prepared by the chlorhydric acid process magnetite in regular octahedra; magnoferrite Fe_2O_3, MgO in octahedra; periclase in colorless or greenish octahedra; haussmanite in square octahedra; protoxyd of manganese in beautiful emerald green octahedra or cubo-octahedra; specular iron in beautiful crystals like the Elba ore. In the last case the current of gas must be slow and regular otherwise the

sesquioxyd of iron is entirely converted in sesquichlorid. Deville suggests that gaseous emanations, as for example chlorhydric acid, may play an important part in geological phenomena and may conduce to the formation of many crystalline minerals.—*Comptes Rendus*, lii, 1264, liii, 161, 199.

W. G.

9. *On a new characteristic of the so-called Semi-Metals;* by Prof. Jerome Nicklès. (Communicated by the author.)—The so-called semi-metals stand between the metals and metalloids marking the transition between these two classes of elements. They share with the first: 1. The metallic lustre; 2. Conductivity of heat; 3. Conductivity for electricity; 4. Density.

With the metalloids they possess the property; 1. of being acidifiable; 2. of forming only feeble salifiable bases; 3. of combining easily with the metals in the manner of an electro-negative body; 4. some of them form a gaseous compound with hydrogen.

These characters are not absolute, and under them the semi-metals may vary among themselves as much as they differ from other elements—but notice a consideration which enables us to determine nearly where the series of semi-metals begins and ends.

The idea of *malleability* is the one which attaches itself most forcibly to our notice of a metal. The word metal involuntarily recalls a body sonorous, heavy, capable of being hammered and drawn into leaves and wire or extended in the rolls.

Viewed from this side we find certain of the metallic elements which possess neither malleability or ductility, and strangely enough these elements are those which we know as *acidifiable metals.* Among them we find *tellurium*, *tungsten*, *osmium*, *arsenic*, *antimony* and lastly, *bismuth*, which only lately passed among the metals, but which has lately fallen from that rank since the establihsment of its isomorphism with antimony and arsenic—themselves isomorphous with phosphorus and nitrogen.—[*Comptes Rendus de l'Acad. des Sci.*, T. L, p. 872, and T. LI, p. 1097. 1860.]

Bismuth has in fact all the external characters of the metals, saving in its want of tenacity and its brittleness, peculiarities common to all the other elements of a metallic lustre which we call semi-metals.

Wanting tenacity, these elements ought consequently to possess little elasticity and sonorousness; but these characters are less obvious and require experiments to determine them, while it is easy to recognize the character of brittleness and want of tenacity.

We propose therefore to consider as *semi-metals* those metallic elements which are neither ductile or malleable, in other words, the *brittle metals.*

Nancy, 23d August, 1861.

Technical Chemistry.—

10. *On the Coloring Matters Derived from Coal-Tar;* by Mr. H. W. Perkin, F.C.S. (A lecture delivered before the [London] Chemical Society on Thursday, May 16, 1861); from the [London] Journal of Gas Lighting, etc., p. 483, July 2, 1861.—Continued from p. 274.

Bleu de Paris.—This is yet another coloring matter produced under circumstances similar to those which give rise to fuchsine. MM. Persoz, V. de Luynes, and Salvétat, give the following account of its preparation and properties:—"Nine grammes of bichlorid of tin and 16 grammes of aniline, heated for thirty hours to a temperature of about 180 centigrade, in a sealed tube, produce neither a red nor a violet, but a very pure and lively blue. This blue, which resists acids, darkens in color by alkalies, but passes to a groselle violet when submitted to this agent in a concentrated state. It preserves its beauty of color by artificial light, and it dyes animal fibres of a shade whose beauty leaves nothing to be desired." I repeated the above experiment twice, and was inclined to give up in despair, for, instead of a fine blue, I obtained nothing more than a dirty-green product. But, from this unpromising product, I at last succeeded in obtaining this beautiful blue coloring matter, and found it to possess all the properties mentioned above. MM. Persoz, V. de Luynes, and Salvétat, have lately given a more particular account of this coloring matter. They describe it as crystallizing from the alcohol solution in the form of fine needles, having an aspect similar to that of ammoniacal sulphate of copper. It is soluble in water, alcohol, wood-spirit, and acetic acid; but insoluble in ether and bisulphid of carbon. With concentrated sulphuric acid, it forms an amber-colored solution, which water converts into a magnificent blue liquid. Strong nitric acid decomposes it; chromic acid precipitates it from its aqueous solutions without decomposition; chlorine destroys it; sulphurous acid does not decolorize it. I found that sulphid or ammonium is also without action upon it. It is precipitated from its aqueous solutions by alkalies and saline compounds.

Aniline Green, or Emeraldine.—Most chemists who have worked with aniline in the laboratory must have noticed the peculiar green-colored substance which forms on the outside of the various species of chemical apparatus that have been standing in the vicinity of any quantity of this body. This product is aniline green. Aniline green has been known for several years. It may be formed by various processes. One process consists in oxydizing aniline with chloric acid. This is effected by mixing a hydrochloric acid solution of aniline with chlorate of potassa. It may also be obtained by oxydizing a salt of aniline with perchlorid of iron. Obtained by either of these processes, it presents itself as a dull green precipitate which, when dried, assumes an olive-green color. It is insoluble in water, alcohol, ether, and benzole; sulphuric acid dissolves it, forming a dirty purple-colored solution from which it is precipitated unchanged by water. With alkaline solutions, it changes to a deep color somewhat similar to indigo: but acids restore it to its original color. The color of aniline green is much enlivened by the presence of an excess of acid; but, unfortunately, as soon as this acid is removed, it passes back to its normal color.

The bases tolzidine, xylidine, and cumidine yield coloring matters under the influence of oxydizing agents; and also when submitted to the action of reducible chlorids at high temperatures, analogous to those obtained from aniline under similar circumstances; but the results generally are not so good, the color of the products becoming tinged with brown as the bases get higher in the series.

Nitroso-Phenyline.—This remarkable body is obtained by the action of nascent hydrogen on an alcoholic solution of di-nitro-benzole. It is represented by the formula $C_6H_6N_2O$. This body is almost insoluble in water, but soluble in acids, and in alcohol, producing crimson-colored solutions, but its color is not nearly so brilliant as that of fuchsine. It seems gradually to decompose when kept. I have not tried any experiments with it as regards its dyeing properties.

Dinitraniline.—Dinitraniline is obtained by decomposing dinitro-phenyle citraconamide by means of carbonate of soda. When pure, it crystallizes in yellow tables. It dissolves very sparingly in water, producing a yellow liquid. It has the formula $C_6H_5(NO_2)N_2$. It does not combine with acids or alkalies, although it appears to be more soluble in acidulated than in pure water. Silk can be dyed yellow with dinitraniline.

Nitrophenyline-diamine, or Nitrazophenalimine.—Dinitraniline, when submitted to the action of sulphid of ammonium, changes into this beautiful base, which crystallizes in needles of a red color, somewhat similar in appearance to chromic acid. It dissolves in water, forming a yellow or orange-colored solution like that of bichromate of potassa. Alcohol and ether dissolve it freely. It is not at all similar to fuchsine in its properties, as it forms salts of yellowish brown or grey color; whereas, fuchsine forms crimson ones. I mention this because nitrophenylene-diamine has been spoken of as having properties similar to those of fuchsine. This base possesses the power of dyeing silk a very clear golden-yellow color.

Picric or Dinitrophenic Acid.—This beautiful acid was discovered as early as 1788, by Hausmann. It may be obtained by the action of heated nitric acid on a great variety of substances; the following are the names of some of them:—indigo, aniline, carbolic acid, seligenine, selicylious and salicylic acids, salicin, phloridzin, cumarin, silk, aloes, and various gum resins, it is now prepared for commercial purposes from carbolic acid, and also from certain gum resins. I have prepared it from carbolic acid on the large scale, in the following manner, with success:—As strong nitric acid acts very violently when brought in contact with carbolic acid, I have found it best to use an acid having a gravity less than 1·3, so as partially to convert the carbolic acid, and afterwards to boil it in stronger acid, to change it into picric acid. On diluting the acid solution, the impure picric acid precipitates. To further purify this, it should be crystallized from boiling water. When preparing this product for commercial purposes, it is advantageous to let all the nitrous fumes formed in its preparation, together with a certain amount of atmospheric air, pass over a fresh quantity of carbolic acid. This will absorb them, and at the same time be converted into nitro, or dinitrophenic acid, and consequently diminish the quantity of nitric acid required for its manufacture. When preparing picric acid from carbolic acid, there is always a quantity of yellow, resinous matter produced, and at times a considerable quantity of oxalic acid. The latter is always produced when the acid which is used to finally convert the carbolic acid is too weak, for then it rapidly decomposes the picric acid, yielding carbonic and oxalic acid. Picric acid, when pure and dry, is of a light primrose-yellow color, crystallizing in strongly shining laminæ. It possesses an extremely bitter taste, and dissolves in water with a beautiful yellow color. When digested with protoxyd of iron in the cold, it yields a brown amorphous compound, which dissolves in water with a blood-red color. Picrate of copper is a beautiful yellowish-green color when in solution. If required for dyeing purposes, it may be prepared by mixing a solation of a picrate with sulphate of copper. Picric acid was introduced as a dye about five or six years since, by MM. Guinon, Marnas, and Bonney, eminent silk dyers of Lyons. Many of the cheap products sold as picric acid are of a brown color, and consist of impure di- and tri-nitrophenic acids, and sometimes of this crude product and ground turmeric.*

Rosolic acid.—Runge first noticed this substance in 1834, when studying creosote, but it was almost lost sight of until again observed by Dr. Hugo Müller only a short time since. He accidentally observed that when crude phenate of lime is exposed to a moist, heated atmosphere, as that of an ordinary drying-stove, it gradually changes in color, and assumes a dark-red tint. This coloration is owing to the formation of rosolate of lime. I have lately heard that phenate of soda undergoes a similar change. Dr. Müller prepared rosolic acid from this product in the following manner:—The crude rosolate of lime is first boiled with a solution of carbonate of ammonia. By this means a crimson solution, containing the rosolic acid, is obtained. This solution is then evaporated nearly to dryness, during which process ammonia is given off, and the crimson-colored liquid gradually changes to a yellowish-red, and at the same time a dark resinous matter separates. This resinous substance is crude rosolic acid. In order to purify it, it is submitted to the following treatment, proposed by Runge:—The crude rosolic acid is dissolved in alcohol, and hydrate of lime added in slight excess. The beautiful crimson solution which is thus formed is agitated for some time with the undissolved portion of the lime, filtered, and the filtrate diluted with water, and, lastly, the alcohol distilled off. The residuary rosolate of lime is then decomposed with just a sufficient quantity of acetic acid, and the whole boiled until every trace of free acetic acid and still adhering alcohol is volatilized. The rosolic acid separates first as a red precipitate, but, when heated, cakes together, forming a dark, brittle substance, having a greenish-metallic lustre.

It may be still further purified by solution in alcohol, to which a little hydrochloric acid has been added, and precipitation with water. Pure rosolic acid is a dark amorphous substance, possessing the greenish-metallic lustre of cantharides. Its powder is of a red, or rather scarlet, shade, which, if rubbed with a hard, smooth body, assumes a bright gold-like lustre. In thin layers, rosolic acid presents an orange color when viewed with transmitted light, but, with reflected light, a golden-metallic appearance. When thrown down from an alcoholic solution with water, it forms a flocculent precipitate of a bright-red color, resembling the basic chromate of lead. Concentrated acids, as acetic, hydrochloric and sulphuric, readily dissolve rosolic acid, forming brownish-yellow solutions, from which water precipitates this acid unchanged. To cold water, it imparts a bright-yellow color, and it is more soluble in hot than cold water. Alcohol and ether dissolve rosolic acid with great facility, forming orange or brownish-yellow solutions, which, on evaporation, leave it in an amorphous state. With ammonia, caustic alkalies, and caustic earths, it forms dark-red compounds, which dissolve with a magnificent red color. These compounds are very unstable. No precipitates are formed with aqueous solutions of the rosolates, with the basic acetates of lead, or with any other metallic salt; nor is any carried down by alumina, or any other metallic oxyd. Dr. Müller made two combustions of rosolic acid, which yielded results that agree with the formula $C_{23}H_{22}O_4$. I and Mr. Duppa, when investigating some of the derivatives of acetic acid, found that when phenic and bromazetic acids were heated together at 120 centigrade, two products were formed, one possessing all the properties of rosolic acid, while the other had the character of brunolic acid. We also found that a mixture of iodine and carbolic acid, when heated with formic, acetic, butyric, or valerianic acids, produce rosolic acids, or a similar substance. A mixture of carbolic acid and iodine did not give a similar result; but a black solid, containing iodine. Rosolic acid has lately been prepared on a large scale, and employed for the purpose of printing muslins. I believe it was a rosolate of magnesia that was employed for that purpose. I do not think it is now used, having been replaced by the more beautiful coloring matter, fuchsine. It was fixed by means of albumen.

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* See the papers of Mr. Lea on Picric acid and its derivatives, in the late volumes of this Journal.

The Coloring Matters of Quinoline or Chinoline.—Chinoline is found associated with lepidine, cryptidine, and other of the higher nitrile bases, in the basic oils obtained from coal-tar, and also in the products obtained by distilling cinchonine with caustic alkalies. It was previously termed leucoline or quinoline; but the substance described under these two names was found to contain three or more distinct bases. These have been studied by C. Greville Williams, who has applied the name chinoline to the product which forms the principal part of the impure substance originally termed quinoline. The substance used for the production of coloring matters need not be pure chinoline, as that product originally termed quinoline is of sufficient purity for this purpose. Cinchonine appears to be the best source of chinoline, yielding, when distilled with excess of caustic alkali, 65 per cent of this body sufficiently pure for manufacturing purposes. Chinoline yields three coloring matters—a violet, a blue, and a green. The following is an account of their preparation by C. Greville Williams:—

"In order to procure the blue color, one part by weight of chinoline is to be boiled for ten minutes with one part and a half of iodid of amyle. The mixture, from being straw-colored, becomes deep reddish-brown, and solidifies, on cooling, to a mass of crystals. This product of the reaction is to be boiled for ten minutes with about six parts of water, and, when dissolved, filtered through paper. The filtered liquid is to be gently boiled in an enamelled iron pan over a small fire, and excess of ammonia gradually added. The ebullition may be prolonged with advantage for one hour, the evaporation of the liquid being compensated for by the gradual addition of weak solution of ammonia. The latter may be prepared by the admixture of equal volumes of ammonia, of the density of 0·880, and distilled water. The hour having elapsed, the whole is allowed to cool, when the color will almost entirely have precipitated, leaving the supernatant liquor nearly colorless. On pouring the fluid away (preferably through a filter, in order to retain floating particles of color), the dish will be found to contain resinous-looking masses, which dissolve readily in alcohol, yielding a rich purplish-blue solution, which may be filtered and kept for use.

"The color prepared as above is, as has been said, of a purplish tint; but, if a purer blue be required, the following modification is to be resorted to. The filtered aqueous solution of hydriodate of amyle-chinoline, is as before, to be brought to the boiling temperature; but, instead of adding ammonia, a solution of caustic potash containing about one-fifth of its weight of solid potash is to be substituted. The addition is to be continued at intervals until three-fourths as much potash has been added as is equivalent to the iodine in the iodid of amyle used. The fluid may, after a quarter of an hour's ebullition, be filtered to separate the resinous color. The product is a gorgeous blue with scarcely any shade of red. On adding the other fourth of potash to the filtrate while gently boiling, a black mass will be precipitated containing all the red, which would otherwise have been mixed with the blue. This mass dissolves readily in alcohol, yielding a rich purple solution, containing, however, an excess of red. The alcoholic solution, on filtration, leaves on a filter a dark mass soluble in benzole, and as sometimes prepared, affording a brilliant emerald-green solution of great beauty. It is not always easy to obtain this green color."

The properties of chinoline violet, and chinoline blue are, as far as I have been able to understand, identical. They are resinous substances which present a coppery appearance by reflected light; but, when in very thin layers, appear of a violet or blue color by transmitted light. They are bases, and dissolved in acids, forming pale-red solutions, which ammonia restores to their original colors. They are slightly soluble in hot water. Tannin precipitates them from their aqueous solution, apparently forming an insoluble compound. Reducing agents do not affect their shade of color.

Of chinoline green, I know but little. Greville Williams describes it as having a brilliant emerald-green color of great beauty. I have observed that, when chlorine is passed through an alcoholic solution of chinoline blue, it changes into green; but whether this is the green spoken of by Williams, I am unable to state.

Naphthaline Colors.—The beautiful hydrocarbon, naphthaline, which has yielded such a long category of substances to the chemist, up to the present time has yielded nothing of practical importance to the dyer. From it the following colored derivatives have been obtained—namely, chloroxynaphthalic acid, perchloroxynaphthalic acid, carminaphtha, ninaphthalamine, nitrosonaphthaline, and naphthamein.

Chloroxynaphthalic and Perchloroxynaphthalic Acids.—These acids were discovered by Laurent. They are produced by digesting their chlorids—namely, the chlorid of chloroxynaphthyle and the chlorid of perchloroxynaphthyle—with an alcoholic solution of hydrate of potassa. They appear to be very difficult substances to obtain in quantity. I have not obtained satisfactory results when endeavoring to prepare them. They have the formula $C_{10}(H_5Cl)O_3$, and $C_{10}(HCl_5)O_3$, respectively. They are regarded with great interest, as being very closely allied with alizarine, the coloring matter of madder; in fact they are viewed as chloralizaric acid. That hypothesis is based upon the idea of alizarine having the formula $C_{10}H_6O_3$; but it happens, very unfortunately for this theory, that the formula of alizarine itself is still a disputed point. Chloroxynaphthalic acid is of a yellow color. It is insoluble in water, and difficultly soluble in boiling alcohol or ether; but it dissolves in concentrated sulphuric acid. This acid is a very sensitive test for alkalies, being changed to an orange-red by them. This may be shown by moistening paper with a weak alcoholic solution of this acid, drying it, and then exposing it to ammoniacal vapors. This will cause it to assume a red color.

The chloroxynaphthalates are described as possessing great beauty, and are of yellow, orange, or crimson colors. The potassium salt is of a red crimson color, and slightly soluble in water; the barium salt crystallizes in silky needles, having a golden reflection. The strontium, calcium, aluminum, and lead salts are of an orange color; the cadmium salt is a vermillion-colored precipitate; the copper and cobalt salts are crimson; and the mercury salt is of a red-brown color. I once dyed some silk with a small quantity of chloroxynaphthalate of ammonia, which I prepared, and found it to produce a good golden-yellow color, of great stability under the influence of light. Perchloroxynaphthalic acid is a yellow, crystalline body, insoluble in water, but soluble in alcohol and ether. With potash or ammonium, it forms insoluble salts of red or crimson color, of great beauty.

Carminaphtha.—This coloring matter was also discovered by Laurent. It is obtained by heating naphthaline with a solution of bichromate of potassa, and then adding sulphuric or hydrochloric acid. It is described as a fine red substance, soluble in alkalies, but precipitated from its alkaline solutions by means of acids. I have never obtained this product when oxydizing naphthaline.

Ninaphthalamine.—Ninaphthalamine is a name which has been given to a remarkable base which was noticed by Laurent and Zinin; but nothing was known of its nature until re-subjected to investigation by Mr. Wood, who has both described and analyzed it, and some of its salts. Its formula is $C_{10}(H_8NO)N$, or naphthalamine, in which H is replaced by NO. Mr. Wood prepares this base in the following manner:—Sulphuretted hydrogen is to be passed through a boiling solution of dinitronaphthaline in weak alcoholic ammonia until nearly all the alcohol has distilled off, which operation should occupy two or three hours. The residue is then to be boiled with dilute sulphuric acid, and filtered. The filtrate, on cooling, deposits an impure sulphate of ninaphthalamine in the form of brownish crystals, which are purified by recrystallization in water two or three times. I have found when crystallizing this salt, that it is best to use water acidulated with sulphuric acid. When pure, this sulphate has to be decomposed with ammonia, and the resulting precipitate of ninaphthalamine washed with water. Thus obtained, ninaphthalamine appears as a bright red-colored crystalline precipitate, which, when viewed under a lens, appears as beautiful needles. It is very soluble in alcohol, producing a solution, which, when dilute, is of an orange color slightly tinged with brown, not nearly so pure in color as that of nitrophenylenediamine. It is slightly soluble in water, and possesses the power of dyeing silk with a color somewhat similar to that of ordinary annata. With acids, it produces colorless salts. Its formula is the same as that of nitroso-naphthaline, though it possesses very different properties. As a dyeing agent, I do not think it would be of any value, even if it could be obtained cheaply.

Nitroso-naphthaline.—This peculiar body is a product of the action of nitrous acid on naphthalamine. It is prepared by mixing a solution of hydrochlorate of naphthalamine with nitrate of potassa. From this mixture it separates as a reddish-brown precipitate. This, when washed with water on a filter and then dried, is dissolved in alcohol, filtered and evaporated to dryness on the water-bath. Thus prepared, it is a crystalline, dark-colored substance, having a greenish-metallic reflection. It is soluble in alcohol, and also in benzole, forming orange-red solutions. When acids are added to an alcoholic solution of nitroso-naphthaline, it immediately assumes a most beautiful violet color, as fine as any aniline purple. Alkalies restore it to its original color. Silk may be dyed a beautiful purple shade with this substance, provided a certain quantity of hydrochloric acid, or sulphuric acid, be present. But what is most unfortunate is, that when the silk thus dyed is rinsed in water, the color immediately passes back to that of the pure nitroso-naphthaline, and also that the amount of acid required to keep up the purple shade, if left in the silk, rots it in a few days. Could this purple be fixed, nitroso-naphthaline would be a cheap and most useful dye. I have endeavored to produce the sulpho-acid of nitroso-naphthaline, thinking that if such a compound could be obtained, it would possess a purple color because it would be an acid itself. But, although sulphuric acid does dissolve it, forming a blue solution, yet no combination takes place. I also endeavored to produce this desired result by treating sulpho-naphthalamic acid with nitrous acid, but obtained only nitroso-naphthaline, the acid of the sulpho-naphthalamic acid having, apparently, separated.

Naphthamein.—Piria observed that naphthalamine and its salts produced blue precipitates, afterwards becoming purple, when brought in contact with perchlorid or iron, terchlorid of gold, nitrate of silver, and other oxydizing agents. This product of oxydation, he terms naphthamein. It is prepared by adding a solution of perchlorid of iron to a solution of hydrochlorate of naphthamein. This mixture gradually changes and becomes blue, and after the lapse of a short time deposits a blue precipitate. This, when separated by means of a filter, is washed with water, which causes it to change in color until of a reddish-brown purple. The filtrate from this substance contains protochlorid of iron, and, according to Piria, chlorid of ammonium. Naphthamein, when heated, fuses and decomposes, leaving a residue of charcoal behind. It is insoluble in water, sparingly soluble in alcohol, but more soluble in ether. It forms a blue solution with concentrated sulphuric acid, and is reprecipitated from it by water. It dissolves in concentrated acetic acid with a purple color, and is not precipitated from this solution by means of water. Silk and cotton may be dyed with it, but the color of this compound is so inferior as to render it useless as a dyeing agent.

Tar Red.—This coloring matter was discovered by Mr. Clift, of Manchester, in 1853. It is obtained by exposing a mixture of the more volatile parts of the basic oils of coal-tar and hypochlorite of lime to the air for about three weeks. Of the pure coloring matter I know nothing, except that, with tannin, it forms an insoluble, or difficultly-soluble, substance. With different mordants, it yields different colors. It seems probable that this coloring matter is derived from pyrrole.

Azuline.—This substance, which is a beautiful blue dye, has been introduced within the last six months. It was discovered by MM. Guinon, Marnas, and Bonney, of Lyons, who keep the process for its preparation a secret. It is obtained from coal-tar, but from which of its numerous derivatives is not known. This coloring matter is a brittle, uncrystallizable body, possessing a coppery, metallic reflection. It is very difficultly soluble in water, but soluble in alcohol, producing a magnificent blue solution, having but a slight tinge of red. With concentrated sulphuric acid it forms a blood-red liquid, which, when poured into an excess of water, precipitates the coloring matter unchanged. Dilute acids have no effect upon azuline. Its alcoholic solution, when mixed with an alcoholic solution of hydrate of potassa, also changes to a dull-red color. This, when diluted with water, forms a purple liquid, which is gradually restored to its original blue color by hydrochloric acid. With excess of ammonia, the solutions of azuline change to a reddish-purple color. This ammoniacal solution, when treated with sulphid of ammonium, gradually assumes a dull, yellowish-brown color. Iodine destroys the color of azuline. In color, it is not quite so fine as chinoline blue, though far superior to Prussian blue.

[The remarks of Mr. Perkin on the methods of applying coal-tar colors in the arts of dyeing and calico printing we omit, as all who are interested in the methods will seek fuller details in the dye-house.—Eds.]

Application of Nitrosonaphthaline.—If cloth is printed with a thickened solution of a salt of naphthalamine, dried, and then passed through a solution of nitrate of potassa, nitrosonaphthaline will rapidly make its appearance as a reddish-orange color; but, unfortunately, the color thus obtained will not well resist the action of soap.

Of the numerous coloring matters of which I have briefly spoken, there are only four that are at present employed by the dyer and printer, namely, aniline purple, fuchsine, picric acid, and azuline; but I think it probable that others of them will soon be introduced, such as the bleu de Paris; and nitrophenylene diamine might be used for silk-dyeing, as its color is good and it stands the action of light well. Unfortunately, the chinoline colors, though very beautiful, are most fugitive. There has been an endeavor to introduce the chinoline blue of late; but, although a considerable quantity of silk was dyed with it at first, it is now scarcely used, because, when exposed to the sun for two or three hours, the dyed silk becomes bleached. Aniline purple resists the light best. Fuchsine and alpha aniline purple soon fade, especially when on cotton. Azuline and bleu de Paris are not easily acted upon by light when on silk.

When the coloring matters of coal-tar were first introduced, there was a great fear that the workmen engaged in their manufacture would suffer in health. All I can say is that, during the few years I have had to do with this branch of manufacture, there has not been a single case of illness among the workmen that has been produced by any operations carried on for the production of aniline purple.

[*Note.*—To the reader desirous of consulting a more detailed account of "the Coloring Matters derived from Coal-Tar" than is contained in Mr. Perkin's instructive lecture, we would commend a series of papers, ***Sur la Préparation des matières colorantes artificielles***, by Prof. Émile

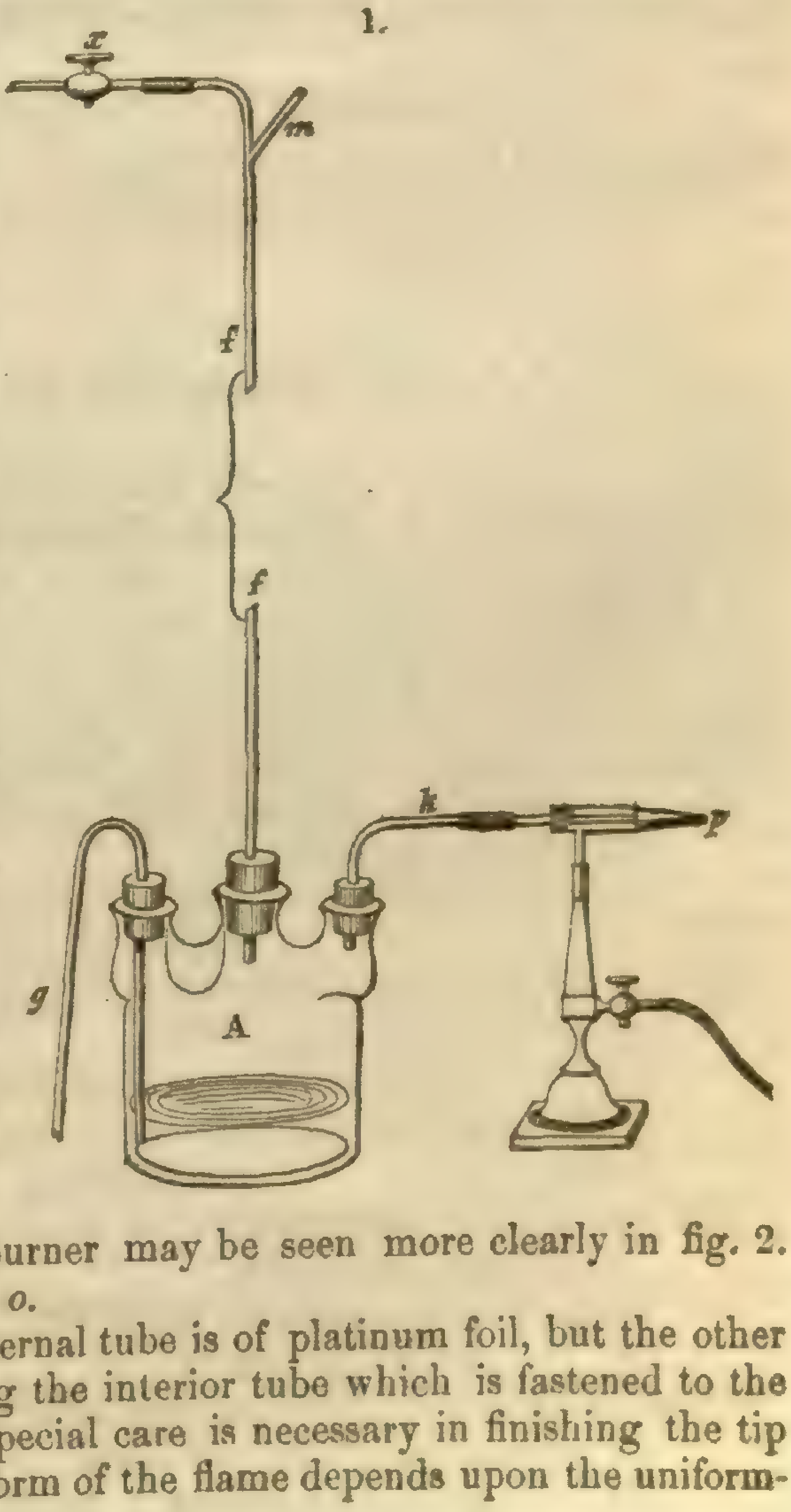
Kopp, which have been published during the past year in Quesneville's *Moniteur Scientifique.* Not only do these articles furnish to the professed chemist a complete historical and critical account of the subject, but they also contain a full discussion of its technical details. Since the very completeness of Kopp's memoirs must necessarily preclude the idea of transcribing them to our pages, we would here once for all call attention to their excellence.]

11. *Blast for Laboratory use.*—Dr. H. Sprengel has devised a combination of the Catalan-trompe and Maugham's burner which appears to merit the attention of all chemists who have at command an abundant supply of falling water and street gas.

The author specially insists upon an apparatus intended as a substitute for the common mouth blowpipe, but remarks that it may be modified so that it can serve to heat crucibles, for the fusion of silicates, etc.

In fig. 1 the cock *x* controls the supply of water falling down the tube *f* in which it mixes with air drawn in through the side tube *m*. The tube *f* should be from 1 to 1½ metres long [1 metre = 39·37 inches] and from 6 to 8 millimetres wide, the syphon *g* being of similar diameter. The capacity of the flask A is about 1 litre [1 quart = 1·14 litre]; it must be provided with tightly fitting corks. Water being allowed to flow by opening the cock *x*, a mixture of air and water reaches the flask A in which separation occurs, the water flowing out through the syphon *g* while air is forced out of the tube *k* by the pressure of the new portions of water and air arriving by *f*. The current of air flowing through *k* is conducted to the centre of a flame of street gas burning, at *p*, from the opening of a Maugham's burner.

1.

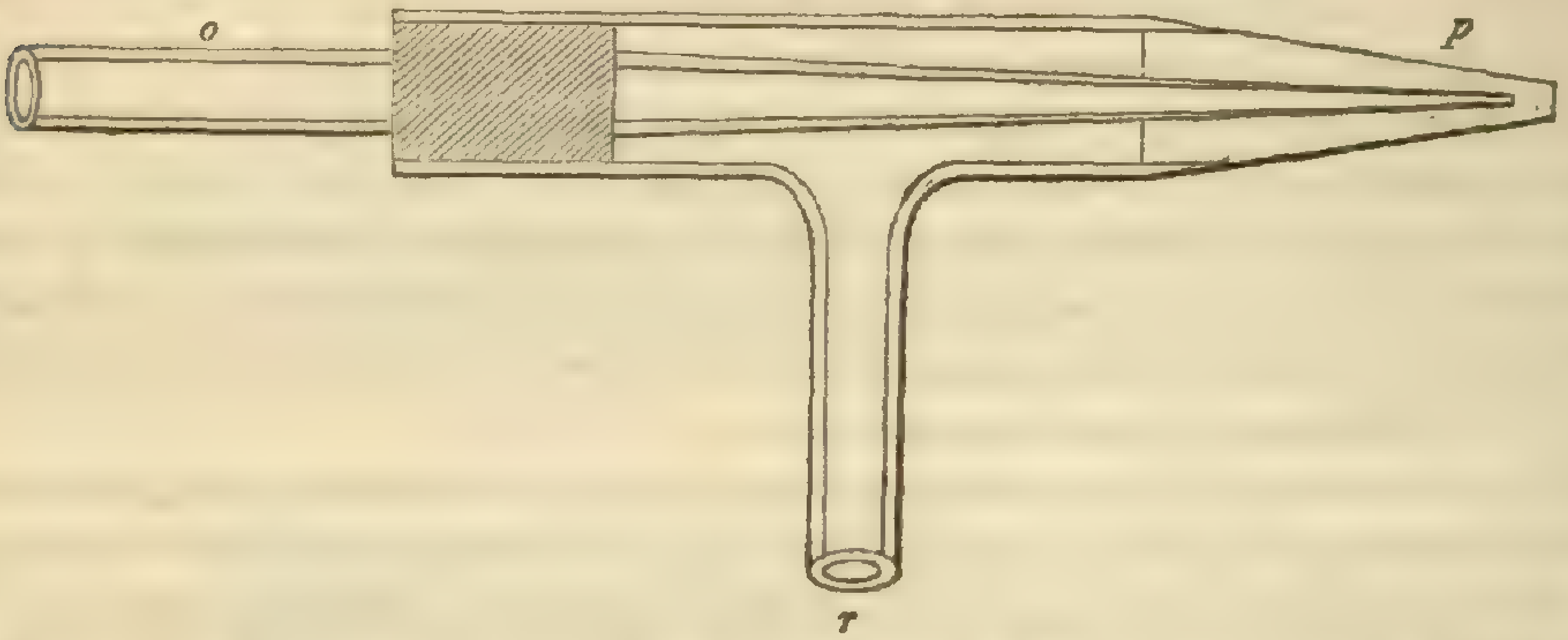


The arrangement of the burner may be seen more clearly in fig. 2. The gas enters at *r*, the air at *o*.

The conical tip *p* of the external tube is of platinum foil, but the other portions are of glass, including the interior tube which is fastened to the other by means of a cork. Special care is necessary in finishing the tip since the beauty and regular form of the flame depends upon the uniformity of the fine opening.

The amount of water to be employed depends of course upon circumstances. With an apparatus of the given dimensions from 25 to 70 litres of it were used according as the flame was of moderate size, or the apparatus driven to its utmost capacity.—(Poggendorff's *Annalen*, cxii, 634).

2.



PHOTOGRAPHY.—

12. *A rapid Collodion.*—The "Photographic Notes" publishes the following formula for a collodion ten times more rapid than ordinary collodion, which is copied without comment in the Moniteur de la Photographie.

Plain collodion, - - - -	30 grammes.
Acetate of soda, - - - - -	0·250 "
Iodid of cadmium, - - - -	0·250 "

This formula we have carefully tried and in our hands it proved absolutely worthless. E. E.

13. *A rapid dry process.*—Le Moniteur de la Photographie for August gives a formula of Mr. Roman de Wesserling modifying *Taupenot's process* and giving a dry collodion process as rapid as the wet process.

It is essential to have a gun-cotton which gives a transparent collodion, and one also which is able to resist the repeated washings. The collodion must be neutral. The ether ought to be sp. gr. 0·732 and should not redden litmus paper, even after long immersion.

For landscapes, and copying, a thick collodion is first made as follows—

1·5 gramme gun-cotton.
90 cubic centimeters ether sp. gr. 0·732.
10 " " alcohol 95 per ct.

Of this thick collodion take 40 parts and add

35 parts ether, sp. gr. 0·732.
25 " alcohol 95 per ct. containing
0·250 grammes iodid of ammonium.
0·250 " " cadmium.

For portraits the formula is varied, viz.—

1 gramme gun-cotton.
90 parts sulph. ether sp. gr. 0·732.
10 " alcohol 95 per ct.

Carefully filter the collodion, then take

50 parts of this thick collodion,
25 " alcohol containing 5 per ct. iodid of cadmium.
10 " " " " bromid of cadmium.
15 " ether sp. gr. 0·732.

The sensitizing bath is

6	grammes	pure nitrate of silver
100	"	" water.

Albumenize with

110	grammes	albumen.
50	"	water.
·050	"	iodid of ammonium.
·025	"	bromid of ammonium.

Shortly before exposing the plate it is again sensitized in a bath composed of—

8	grammes	pure nitrate of silver.
8	"	glacial acetic acid.
100	centimeters	pure water.

It is afterwards slowly dipped in fresh water and set up in a dark place to dry.

After exposure develop by a bath of—

100	grammes	water.
0·5	"	pyrogallic acid.
10	"	acetic acid.

This developing bath should be heated to 120° Fah. Strengthen by re-development with pyrogallic acid. Fix as usual. E. E.

II. GEOLOGY.

1. *On the Taconic System of Dr. Emmons;* by T. Sterry Hunt, M.A., F.R.S.—Dr. Emmons while engaged in the survey of a part of the State of New York, recognized the existence of a series of sedimentary rocks, which he described under the name of the Taconic system, and regarded as older than those supposed by his colleagues to represent the Silurian series. A similar view had been maintained by Eaton, but was rejected by most of the American geologists, who up to this time have regarded these Taconic rocks of Emmons as belonging to the Lower Silurian series. In 1844 Dr. Emmons described certain fossils from these rocks, which he supposed to be new and to distinguish what he called the Taconic system, regarded by him as the true palæozoic base. In 1846 Mr. Barrande discovered in Bohemia, beneath the horizon of the hitherto recognized Silurian fossils, a new and extensive fauna in what he designated the Primordial Zone. The fossils described by Dr. Emmons consisted, besides some imperfect trilobites, of a few graptolites, mistaken by him for fucoids, and several very doubtful forms which are valueless for the purpose of determination. According to Dr. Emmons this system, which he divides into an upper and lower portion, has a thickness of 30,000 feet, and extends throughout the whole Appalachian chain. He has described it as composed in ascending order of, 1. Granular quartz. 2. The Stockbridge limestone. 3. Magnesian slates. 4. Sparry limestone. 5. Roofing slates (graptolitic). 6. Silicious conglomerate. 7. Taconic slates. 8. Black slates. This is not their apparent order of superposition, but Dr. Emmons conceives that the whole series has been inverted since its deposition. In fact the schistose strata 5, 6, 7 and 8, pass successively beneath the magnesian slates and limestones, which in their turn are overlaid to the east by the

Green Mountain gneiss. This latter formation Dr. Emmons regards as a primitive azoic rock, upon which were successively deposited the members of the Taconic system, commencing with the quartzite, which forms its base, and crowned by the black and Taconic slates, which are now, from an immense overturn, placed at the bottom of the series, while the ancient gneiss lies at the top. It is hardly necessary to say that this supposition is wholly unwarranted by the facts. In a paper on American geology already cited we have shown that the apparent succession of the rocks of the Quebec group is the true one. The black slates are really at its base and successively overlaid by the conglomerates, roofing slates, limestones and quartzites, and the gneiss is a newer rock, being no other than the Sillery sandstone in an altered condition, and as we have there shewn, entirely distinct from the Laurentian gneiss. Mr. Emmons has fallen into an error, similar to that of Prof. Nichol with regard to the gneiss of the Scottish Highlands, so well refuted by Murchison, Ramsay and Harkness, and has consequently been driven, in order to explain the structure of the Green Mts. to admit not merely an upthrow with Nichol, but a complete overturn of the whole palæozoic series in question. As to the geological age of this series, Dr. Emmons maintains that his Taconic system occupies a position inferior to the Champlain division of the New York system, and is consequently beneath the Lower Silurian system of Murchison. As we have before shown, however, the fossils of the Quebec group show it to be the palæontological equivalent of the Calciferous sandrock. The Stockbridge and sparry limestones, with their accompanying slates (excepting only 7 and 8,) we conceive to be no other than the Quebec group, of which they have both the stratigraphical position and the lithological characters. Dr. Emmons has maintained that limestones of the age of the Calciferous are found overlying the black slates, and has appealed to this in proof of the antiquity of the whole series, of which he imagined these slates to form the summit, but inasmuch as these slates are really older than the Quebec or Calciferous strata, his argument falls to the ground. Mr. Billings has lately found *Conocephalites* in the red sandrock of Highgate, Vermont, which is supposed to overlie the black slates in question. As this primordial genus occurs also in the Potsdam sandstone of Lake Champlain, the question arises whether these slates are palæontologically distinct from the Potsdam, or are only its deep sea equivalent, sustaining to the littoral formation of quartzose sandstone on Lake Champlain, the same relation as the great Quebec group does to the Calciferous sandrock of the New York geologists. Dr. Emmons claims that the whole of his Taconic system is inferior to the Potsdam sandstone, which is the admitted base of the Champlain division, but we have already shown that the whole of his system, with the probable exception of these slates, is of the age of the Calciferous sandrock, the second member of that division.* Unless then these lower black slates contain a fauna distinct from and older than that of the Potsdam sandstone, there remains absolutely nothing of the Taconic system which Dr. Emmons placed below the base of the Champlain division, that is to say, below the Potsdam sandstone. If, however, as is probable, these slates contain a fauna distinct from the Potsdam, they might be retained under the name of the

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* This Journal, May, 1861, p. 401.

Taconic formation, as a lower member of the Primordial Zone, to which the Potsdam sandstone unquestionably belongs.

These lower slates in Georgia, Vt., have as already remarked, furnished certain trilobites of primordial type which Mr. James Hall has described under the name of *Olenus Vermontana* and *Olenus Thompsoni*, though they are provisionally referred by Barrande to the genus *Paradoxides*. In the meantime the only trilobite as yet met with in the typical Potsdam sandstone of this region, which is rarely fossiliferous, is *Conocephalites.** A collection of fossils recently made by Mr. James Richardson in exploring the Straits of Bellisle for the Geological Survey of Canada, fortunately furnishes the means of determining the relations of the trilobites described by Mr. Hall. On the north side of the Straits he found reposing on the Laurentian rocks a coarse reddish sandstone holding *Scolithus* like that from the Primal sandstone of Pennsylvania. Resting upon this, and dipping gently southward, is a limestone in which occur both *Olenus Thompsoni* and *O. Vermontana*, with what appears to be an *Arionellus*, besides *Obolus*, *Capulus*, and a large spirally marked coral resembling *Zaphrentis*. These rocks, which evidently represent the Primordial Zone, are overlaid by others containing the characteristic fossils of the Calciferous sandrock and the compound graptolites of the Quebec group. These primordial trilobites then overlie the sandstone with *Scolithus*, but as we have elsewhere observed, that species appears unlike the *Scolithus* from the Potsdam of Lake Champlain, and should not be too much relied upon for fixing the geological age of this formation. It is not improbable that the true equivalent of the Conocephalites and Lingula sandstones of Lake Champlain will be found in some of the strata above the Olenus beds of Bellisle.

We have seen that Emmons, guided by a false notion of the age of the Green Mountain gneiss which led him to admit an inversion of the whole series, placed the shales which form a portion of the Primordial Zone high in the second fauna, above the whole Quebec group. On an entirely different ground, Hall assigned the shale containing *Olenus*—two species of which genus he described in 1847 in the 1st Vol. of the Palæontology of New York,—to the Hudson group. In this, as Barrande shows, Mr. Hall felt himself justified by the authority of Hisinger, who in his great work on the fossils of Sweden, *Lethæa Suecica*, 1837, gives the succession of palæozoic rocks in Sweden as follows in ascending order; 1. Fucoidal sandstone; 2. Orthoceratite limestone; 3. Alum slates with *Olenus;* 4. Argillaceous slates with graptolites, etc.

The *Olenus* slates, said by Hisinger to overlie the orthoceratite limestone, (corresponding to the Trenton,) Mr. Hall unhesitatingly regarded as the equivalents of the Hudson group, in which *Olenus* was to be looked for as a characteristic fossil, and hence the strata containing these trilobites were, on the authority of Hisinger, regarded as belonging to the summit of the second fauna. In reality however this order assigned by Hisinger to the formations of Sweden is false, since the alum slate with *Olenns* lies below, and the graptolitic slate above the orthoceratite limestone. This error of Hisinger is the more strange since he had long before, as Barrande shows, indicated the true succession of these rocks, and it is perhaps a mistake of the copyist or printer; it is the more to be regretted as his authority had caused it to be adopted by Mr. Hall in America. (*Geol. of Lake Superior*, *Foster and Whitney*, II, pp. 298–318.) The alum slate with the underlying sandstone represents in Sweden the primordial zone.

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* Mr. Barrande refers to three species of *Dikellocephalus* indicated by Dr. Bigsby as occurring in the Potsdam of New York. It will be seen by referring to his memoir (Quar. Jour. Geol. Soc. 1858, p. 339, compared with p. 420,) that Dr. B. alludes only to the existence of these species as described by Owen in the Mississippi valley.

To Dr. Emmons undoubtedly belongs the merit of having recognized for the first time the trilobites which are known to belong to the primordial zone, although from incorrect notions of stratigraphy he placed the slates containing them at the summit of the series of rocks to which he gave the name of the Taconic system. We have shown that the true place of these shales is at the base of the series, and that the remainder of the Taconic system is the palæontological equivalent of the Calciferous sandrock; it is not yet certain whether these lower shales with a primordial fauna do not sustain a similar relation to the Potsdam sandstone, in which case the whole of the Taconic system would be the equivalent of the two lower groups of the Champlain division. It yet remains to be seen whether Dr. Emmons can retain from the wreck of his system, the lower slates as a Taconic formation older than the Potsdam sandstone of Lake Champlain, and subordinate to the Primordial Zone, whose fossils he was the first to recognize.

2. *Contributions to Palæontology*, being Descriptions of New Species of Fossils from the Upper Helderberg, Hamilton and Chemung Groups. Albany. August and September, 1861. 8vo. pp. 84.—We have received this valuable paper at too late an hour for critical notice in our present issue. It is, as the Author states in a prefatory note, "A Continuation of Appendix C of the Fourteenth Annual Report of the Regents of the State Cabinet, published July, 1861, and contains descriptions of new species of GASTEROPODA, CEPHALOPODA and CRUSTACEA, with notices of some of the species before described from the same strata."

"The first twenty-four pages were published and distributed in August, and the remaining portion is published in September according to the imprint at the bottom of each page. The entire paper, with illustrations, will be republished in the Fifteenth Annual Report of the Regents of the State Cabinet; and the Author will endeavor to furnish those who may receive the accompanying pages, with a copy of the illustrations when published." JAMES HALL.

Albany, Sept., 1861.

This is as it should be. The criticisms we took the liberty to make on the Thirteenth Annual Report and its Palæontological Supplement (see this Jour., [2] xxxi, p. 292), were based upon the want of precisely this definite statement of dates of publication. It will be satisfactory to all naturalists to know that the suggestion has been met so promptly by this distinguished Palæontologist. We shall take another occasion to discuss the contents of Prof. Hall's memoir, as well as the 14th Annual Report of the Regents of State Cabinet. The first twenty pages of the Appendix C in which, form as above stated a part of Prof. Hall's Contributions.

III. ZOOLOGY.

1. *Synopsis of a Course of Lectures given at the Museum of Natural History (Paris), in* 1850; by M. Isidore Geoffroy Saint-Hilaire.*

[The following synopsis, although by no means new, will be read with interest at the present time in connection with the recent discussions on the origin of species, especially as it is not generally accessible to American students.]

ON SPECIES.

I. *On the Animal Series, and the Parallellic Classification.*

(1.) The various zoological types may be reduced to a serial or progressive order.

(2.) The principle of coördination of the series resides essentially, not as was vaguely said before the time of Lamarck in the greater or less *perfection* or *complication* of organization, but in its *diversification*, *specialization* and *centralization*, which are at the maximum at one extremity of the series, at the minimum at the other.

Thus, at the top, the beings whose apparatus, organs, and tissues are the most *diversified*, whose functions are the most *specialized*, whose organism is the most *centralized:* at the bottom, the beings whose composition is the most *homogeneous*, in whom the functions are the most completely confounded, and the life, in some sort *diffused.*

(3.) In the animal series, sometimes the terms succeed each other at very close intervals, occasionally even, without sensible interval; sometimes two consecutive series remain at a great distance from each other.

The series is then neither *regular* nor *continuous.*

(4.) Neither is it *simple.* Often, and even most usually, it is double, triple, or more complex still, successions of terms manifestly analogous being found in two or several groups otherwise distinct. These successions of analogous, or as they would be better called, *homologous* terms in different groups, are what we have named *parallel series.*

(5.) Hence arise double relations which it is important to know and to express. Great attention has always been paid to the *affinities* which unite the *varied types* comprised in the *same group;* the knowledge of the affinities which *bind together* the *homologous types* existing in *different groups*, is not less necessary to the rational conception of the series and to the expression of their natural connections.

(6.) This expression, it has seemed to us, can be given by the new system of classification, known as the *parallellic classification*, or classification by *parallel series*, which is on the whole, but a very simple improvement on the system usually employed.

Suppose a group *n*, comprising several secondary types which we shall designate by the letters *A, B, C, D, E.*

Suppose another group *N*, holding the relations with the first which we have just indicated, that is to say, of which the secondary types are homologous to the preceding. We shall call them, to express at once the *continued* difference and the homology, *a, b, c, d, e.*

Suppose a third group **n**, giving similarly a, b, c, d, e. A fourth N, giving A, B, C, D, E, and so on.

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* **Translated for this Journal by a lady.**

It is manifest that the expression of the multiplied relations, existing between all these terms will be obtained, if on the one hand, the terms of each series, *A*, *B*, *C*, &c., *a*, *b*, *c*, &c., follow each other without the intercalation of any foreign term; and on the other, if the homologous terms of the various series, *A*, *a*, a, *B*, *b*, b, &c., are placed in apposition to each other. The parallellic classification satisfies these two conditions by the following combination, simple enough to be seized at a glance:

A	*a*	a	A
B	*b*	b	B
C	*c*	c	C
D	*d*	d	D
E	*e*	e	E
F	*f*	f	F[*]

(7.) There exist, so to speak, parallelisms of all degrees. The species of one and the same genus, the genera of one and the same family, often form parallel series; it is sometimes thus (to go no higher) with the classes of the same branch. The word *type*, just now employed, may then receive any given value, provided that by *group* is understood a division of the degree immediately superior.

The *parallellic classification* has been, for eighteen years, (in 1850) applied by various authors to the greater part of the branches of Zoology, Anthropology, Feratology and Botany.

II. *Summary of lessons on the question of species.*

(1.) The characters of *species* are not *absolutely fixed*, as many have said, still less indefinitely varied, as others have maintained. They are fixed for each species, so long as it is perpetuated in the midst of the same circumstances. They become modified if the surrounding circumstance change.

(2.) In the latter case, the new characters of the species are, so to speak, the *resultant* of two contrary forces; the one *modifying*, is the influence of the new surrounding circumstances; the other *conservative* of the type, is the hereditary tendency to reproduce the same characters from generation to generation.

In order that the *modifying influence* should predominate, in a very marked manner, over the *conservative tendency*, it is necessary then that a species should pass from the circumstances in the midst of which it was living, into a new and very different totality of circumstances; that it should change, as has been said, its *surrounding world*.

(3.) Hence the very narrow limits of the variations observed amongst wild animals.

Hence also the extreme variability of domestic animals.

(4.) Amongst the former, species remain generally in the places and the conditions where they are established, or remove from them as little as possible; for their organization is in harmony with these places and conditions, and would be in disagreement with other surrounding circumstances.

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[*] For greater simplicity we have here supposed the four series equally extended, and without gaps. In reality it is scarcely ever so. For example, there might be *A*, *B*, *D*, *E*, *F*; *a*, *c*, *d*, *f*, &c. The series are not the less manifestly parallel, only, some terms remain without homologues.

The same characters must then be transmitted from generation to generation.

Circumstances being permanent, species are so likewise.

(5.) Already, however, the permanence, the fixity are not absolute. The gradual expansion of a species on the surface of the globe is, at length, the necessary consequence of the multiplication of individuals. Other causes, of an order less general may also bring about partial displacements. Whence, especially at the limits of the geographical distribution of the species most extended, arise notable differences of habitat and climate, which, in their turn lead inevitably to secondary differences of regimen and even of habits. To these various kinds of differences correspond *races* characterized by modifications in color and other exterior characteristics, in proportion and form, and sometimes even in internal organization.

(These races have been arbitrarily considered, sometimes as *local varieties*, sometimes as distinct *species*).

(6.) Among domestic animals, the causes of variation are much more numerous and more potent. In a long series of experiments which, though undertaken with a merely practical object, have no small theoretic importance, species of various classes, to the number of about forty, have been constrained, by the intervention of man, to quit savage life, and bend to very different habits, regimen and climates. The effects obtained have been in proportion to the causes; there have been formed a multitude of very distinct races; amongst them several offer characters equal in value to those by which genera are commonly differentiated.

(7.) The return of many of the domestic races to the wild condition has taken place on various points of the globe: thence, a second series of experiments inverse to the preceding, and furnishing the counter-proof. If domesticated animals are replaced in the circumstances in the midst of which their wild ancestors existed, their descendants, after some generations, resume the characters of the latter. They only assume analogous characters if they are restored to savage life under conditions analogous, but not identical.

(8.) To resume: the *observation* of wild animals already demonstrates the *limited* variability of species.

The *experiments* on wild animals domesticated, and on domestic animals returned to the wild condition, demonstrate it more clearly still.

The same experiments prove, moreover, that the differences produced may be of *generic value.*

(9.) The truth or error of a doctrine may almost always be brought to light by the value of the consequences derived from it.

The *theory of limited variability* may lead to *rational* solutions with respect to questions completely insoluble for the partisans of absolutely fixity, or which the latter can only resolve by aid of the most complex and improbable hypotheses.

(10.) It is thus with the fundamental question of anthropology. The common origin of the various human races is *rationally* admissible in the point of view of variability, and *in this point of view alone.* In order to admit it with us, the partisans of *fixity* have been forced to conclude against their own principle.

(11.) In palæontology, to the theory of limited variability corresponds a simple and rational hypothesis, that of *filiation;* to the doctrine of fixity, two hypotheses equally complicated and improbable, that of *successive creations* and that called *translation.*

According to the hypothesis of *filiation,* the existing animals are sprung from *analogous* animals which have lived in the previous geological epoch. We may, for example, seek the ancestors of our elephants, rhinoceroses and crocodiles amongst the elephants, rhinoceroses and crocodiles of which palæontology has shown us the antediluvian existence.

This hypothesis has been rejected as irreconcilable with the *fixity* of species, because of the specific differences which exist between the ancient animals and their modern analogues. To the simple explanation of these differences by changes taking place, from one geological period to another, in surrounding circumstances, has been preferred the hypothesis of several *successive creations,* and, more recently, that of *translation.* To resume the example quoted above: these two hypotheses agree in admitting the complete extinction of the ancient species of elephants, rhinoceroses and crocodiles; but the first replaces them by the elephants, rhinoceroses and crocodiles of the *new creation;* the second, by the actual species, supposed to be pre-existant, with all their present characters, on some other point of the globe which had remained unknown. Of these three hypotheses, that which springs from the theory of variability is incontestably the most simple and least conjectural. For this reason, it may already be presented as the most probable.

(12.) But it has not alone this advantage over the others.

It can be, and is even now, verified in its application to various particular cases.*

(13.) Moreover, it is confirmed by various considerations in presence of which it seems difficult to maintain the other two hypotheses. Without insisting on that of *successive creations,* we shall confine ourselves to placing here in opposition, in two of their consequences, the hypothesis of *filiation* and that of *translation.*

(14.) According to the first, the existing animals should be descended from animals merely *analogous;* according to the second, from animals *similar* to themselves. Now the preservation of the *same characters at all periods,* would suppose the existence, *at all periods* also, of the *same* surrounding *circumstances,* which is *inadmissible.*

(15.) In the hypothesis of *filiation,* the number of species might vary, becoming greater or less from one geological period to another; for if at each revolution, there has been an extinction of a portion of the species, those which remain *must* have undergone modifications, various according to circumstances, and which *may* have acquired the value and permanence of specific characters. In the opposite hypothesis, at each revolution a portion of the species disappears; the others *remain what they were:* they are displaced, but without undergoing organic modifications. Consequently, the extinctions are here *without any possible compensation.* Then, according to this hypothesis, the number of animal and vegetable species must have gone on constantly decreasing; there would be a *pro-*

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* Which has been demonstrated in the Course of Lectures of 1847.

gressive diminution, a depopulation of the globe: the two hundred and sixty thousand animal and vegetable species which, according to the most recent estimations, now cover the surface of the earth, would be but the wrecks of a creation infinitely richer in past times.

Such is the consequence to which the hypotheses of absolute fixity and of translation naturally lead: each will judge how far it agrees with the notions that we possess on the ancient condition of the globe.

(16.) The substitution of the theory of *limited variability* for the hypothesis of absolute fixity renders necessary a new definition of species.

To approach as nearly as possible the definitions most in use, and for the moment, considering only the existing order of things, we shall say:

Species is a collection or succession of individuals characterized by a combination of distinctive features, the transmission of which is NATURAL, REGULAR *and* INDEFINITE *in the existing order of things.*

The possibility of distinction, the natural and regular transmission, stability and permanence equal to those of the present condition of the globe—such are the three essential elements of this definition of species.

A few words are necessary to explain the terms of it.

Hybrids are not generally unfruitful, as has often been said;* they can transmit their characters, *always mixed.* Hybrid races are not propagated with the constancy and regularity observed in species; they soon die out, or return by the effect of crossing, to one of the species whence they sprung. The transmission is then neither *regular* nor *indefinite.*

It is the same with monstrous or anomalous races.

Domestic races approach much more nearly to species. Amongst those which are very ancient, and which have thus acquired a great fixity, the transmission may even be said to be *regular;* it may be *indefinite*, and as durable as the existing order of things, but only by the intervention of man, which is necessary to maintain the races as it was to create them. The transmission is not then natural.

2. *On some Objects of Natural History from the Collection of Mr. Du Chaillu;* by Prof. OWEN.—Prof. Owen's first knowledge of the zoological collection was derived from a letter sent by Mr. Du Chaillu, dated Gaboon, June 13, 1859, and received in the British Museum in August, 1859, in which Mr. Du Chaillu specified the skins and skeletons of the gorilla or n'gena, kooloo-kamba, nschiego, and nschiegombovie which he had collected, offering them for sale, with other varieties, to the British Museum. Prof. Owen replied, recommending the transmission of the collection to London for inspection, with which recommendation Mr. Du Chaillu complied, bringing with him all the varieties he had named, with other objects of natural history, from which he permitted selection to be made. The skins of the adult male and female of the young of the *Troglodytes gorilla*, afforded ample evidence of the true coloration of the species. In the male, the rufo-griseous hair extends over the scalp and nape, terminating in a point upon the back. The prevalent grey color, produced by alternate fuscous and light grey annulations of each hair, extends over the back, the hair becoming longer upon the nates and upon the thighs. The dark fuscous color gradually prevails as the hair extends down the leg to

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* We have given proofs of this that cannot be refuted, by uniting in two tables the indications relative to hybrid Mammifers and Birds, and *to their products.*

the ankle. The long hair of the arm and forearm presents the dark fuscous color; the same tint extends from below the axilla downwards and forwards upon the abdomen, where the darker tint contrasts with the lighter grey upon the back. The scanty hair of the cheeks and chin is dark; the pigment of the naked skin of the face is black. The breast is almost naked, and the hair is worn short or partially rubbed off across the back, over the upper border of the iliac bones, in consequence, as it appears, of the habit ascribed by Mr. Du Chaillu to the great male gorilla of keeping at the foot of a tree, resting its back against the trunk. The skin of the great male gorilla, as mounted in the British Museum, exhibits two opposite wounds,—the smaller in front on the left side of the chest, the larger close to the lower part of the right blade-bone. Two of the ribs in the skeleton of this animal are broken on the right side near where the charge had passed through the skin in its course outwards. These marks correspond with the account of the slaughter of the great gorilla given by Mr. Du Chaillu. Prof. Owen proceeded to describe the color of the female gorilla, which, it appears, was generally darker and of a more rufous tint than the male. In one female the rufous color so prevailed as to induce Mr. Du Chaillu to note it as a red-rumped variety. In the young male gorilla, 2 ft. 6 in. in height, 1 ft. 7 in. in the length of the head and trunk, and 11 inches across the shoulder, the calvarium is covered with a well dressed "skull-cap" of reddish colored hair. The back part of the head behind the ears, the temples and chin are clothed with that mixture of fuscous brown and grey hair which cover with a varying depth of tint the trunk, arms and thighs. The naked part of the skin of the face appears to have been black, or of a very dark leaden color; a few scattered straight hairs, mostly black, represent the eyebrows. A narrow moustache borders the upper lip, the whole of the lower lip and sides of the head are covered with hair of the prevailing grey fuscous color. The rich series of skulls and skeletons brought home by Mr. Du Chaillu illustrate some most important phases of dentition. These phases were specified by Prof. Owen at length. The deciduous or milk dentition, it was remarked, were in the youngest specimen of the gorilla something similar to those of the human child, but an interspace equal to half the breadth of the outer incisor divides that tooth from the canine, and the crown of the canine descends nearly two lines below that of the contiguous milk molar. The deciduous molars differed from those of the human child in the more pointed shape of the first, and much larger size of the second. The dentition of the young gorilla corresponds best with that exemplified in the human child between the eighth and tenth years; the difference, however, is shown in the complete placing of the true molar, whilst the premolar series is incomplete. It was worthy of remark, also, that in both specimens examined the premolars of the upper jaw had preceded those of the lower jaw, and that the hind premolar has come into place before the front one. In the later development of the canines and the earlier development of the second molars of the second dentition the gorilla differs, like the chimpanzee and the orangs, from the human order of dental development and succession. An opportunity of observing this order in the lower races of mankind is rare. Prof. Owen availed himself of the opportunity in the case of the male and female dwarf Earthmen from

South Africa, exhibited in London. He found dentition at the phase indicative of the age of from seven to nine in the English child; other indications agreed with this evidence of immaturity. The children were dressed and exhibited as adults. Both showed the same precedency in development of canines and premolars which obtains in the whole race. Referring next to the variety of the chimpanzee brought by Mr. Du Chaillu from the Camma Country and from near Cape Lopez, Prof. Owen remarked that this species accords specifically in its osteological and hirsute development with the *Troglodytes niger*. It is stated by Mr. Du Chaillu to be distinguished by the natives of Camma as the nschiegombovie from the common chimpanzee (*Troglodytes niger*), called by them the nschiego. From the character of the skins of the male and female specimens of this species brought by Mr. Du Chaillu to London, Prof. Owen would have deduced evidence of a distinct and well-defined variety of Troglodytes.—*Athenæum, Sept.* 14, 1861.

3. *On the Height of the Gorilla;* by Dr. J. E. GRAY.—Much difference occurs in the statements of travellers and others with reference to the height of the great African ape. Bowdich, the first traveler by whom it was mentioned, under the name of *Ingēna,* states it, on the authority of the natives of the Gaboon, to be generally five feet high; but, in some recent notices, it has been asserted to reach the height of six feet two inches; and the specimen exhibited at the meeting of German naturalists at Vienna is said, on good authority, to have measured more than six feet in height. The measurement of a stuffed skin without bones is necessarily delusive, depending as it does, firstly on the mode in which the skin has been originally prepared, and, secondly, on the extent to which the artist may be disposed to stretch it. Such measurements are not to be relied on unless they are in accordance with those of the bony skeleton; and it, therefore, occurred to me that it would be desirable to measure the long bones of the limbs of the different skeletons existing in the British Museum, the osseous structure giving the only certain dimensions on which reliance can be placed. The skeletons in the British Museum are six in number, viz: 1. A skeleton, obtained from Paris by Prof. Owen, and mounted in the best French manner. 2, 3, 4. Skeletons of male, female and young, purchased from Mr. Du Chaillu. 5. A skeleton of a male, purchased at Bristol, of which we have also the stuffed skin. 6. An imperfect skeleton, purchased from M. Parzudaki, of Paris.

	Humerus.	Ulna.	Radius.	Femur.	Tibia.	Fibula.
	Measurement in inches.					
Articulated specimen from Paris, - - - - - - - -	17	14	13	14½	11½	10½
Skeleton from Du Chaillu's stuffed specimen (called the "King of the Gorillas"), - - - - - - - - - -	16¼	14	13¼	13¾	11	9¾
Skeleton of young male, from the specimen purchased at Bristol, - - - - - - - - - - - - - -	14½		11	15		9½
Imperfect skeleton, purchased of M. Parzudaki, - - -	12	11	10	11		9½
Skeleton of female, purchased of Mr. Du Chaillu, - -	13	11	10½	11	9	7
Skeleton of young male, purchased of Mr. Du Chaillu, -	12	11½	9½	10	8½	7

The measurements of the several bones of each of these skeletons are given in the accompanying table.

They were taken by Mr. Gerard with a tape measuring inches and quarters of inches only, but are quite sufficient for a comparison between the specimens themselves, and as affording materials for determining the actual height of the animal. As the largest of these (viz., the Paris specimen, photographed for the Trustees of the British Museum by Mr. Fenton) stands five feet two inches in height, we are justified in concluding that to be in all probability the extreme natural height of the full grown animal.—*Athenæum, Sept.* 14, p. 348.

4. *Cambridge Museum of Comparative Zoology.*—The labors of Prof. Agassiz and the intelligent patronage of the Commonwealth of Massachusetts in establishing the new Museum of Comparative Zoology at Cambride, have found an appreciative eulogist in Prof. Owen in his recent communications to the London Athenæum, on the importance of establishing in London a national museum of Natural History. Prof. Owen had estimated the area required for such a museum as he contemplates, with a forecast of thirty years accessions, to be not less than five acres of land, and he is agreeably confirmed in this estimate by finding that this area is the same allotted at Cambridge to our new museum. We had marked this whole communication for extract, but cannot find space for it at present. Prof. Agassiz in a late letter says, "I am now hard at work perfecting the internal arrangement of the collections which begin to outgrow my strength. We want an *addition* to the present building larger than it is, for more than half the specimens on hand are not yet exhibited."

IV. ASTRONOMY AND METEOROLOGY.

1. *Discovery of the 71st Asteroid.*—August 13, 1861, about 11^h, M. Luther at Bilk near Dusseldorf discovered a new asteroid which appeared as a star of the 11th magnitude, making the number of asteroids now known 71. Its daily motion was $-16'$ in Right Ascension, and $+2'$ in Declination. It was observed at Bonn on the 15th, and at Mannheim on the 17th of August. This planet has been named Niobe.

2. *Re-discovery of Pseudo-Daphne.*—August 27, 1861, M. Goldschmidt of Paris re-discovered the planet which he discovered Sept. 9, 1857, and which has received the name of Pseudo-Daphne. The history of this planet is very remarkable. On the 22d of May, 1856, M. Goldschmidt of Paris, discovered a new planet of the 11th or 12th magnitude. He observed it again on the 25th, but on neither occasion was he able to locate the planet accurately for want of suitable instruments. On the 31st it was observed at Marseilles; it was observed at Berlin, June 1, 2, and 3d, and it was observed at Vienna, June 2d and 4th. The planet being now quite faint and difficult to observe, was no longer followed; so that the reliable observations only embrace an interval of *four days*, and the arc described in this interval was but little more than *one degree.* From this small arc it was required to deduce the elements and compute an ephemeris for the planet's return to opposition in Sept. 1857.

M. Pape of the Altona Observatory computed the best orbit he was able from these observations, and published an ephemeris for the approaching opposition. Anticipating the difficulty of finding the planet, the astronomers at Oxford, Paris, Berlin, Vienna, Altona and Bilk agreed upon a joint search, each observer selecting a portion of the heavens which he would specially explore.

Sept. 9th, 1857, M. Goldschmidt of Paris announced that he had rediscovered Daphne, only about two degrees from the place assigned by the elements of Pape. The planet was afterwards observed at Bilk, Leyden, Bonn, Berlin and Cambridge, and was followed till Sept. 30th. On computing the orbit from the observations of 1857, it was found that the elements differed very materially from those which M. Pape had obtained; and indeed these new elements would not represent the places of Daphne in 1856, within more than 12 degrees. This discrepancy was first announced by M. Schubert, in Sept. 1858; and he of course concluded that the planet discovered by Goldschmidt Sept. 9th, 1857, and which was supposed to be Daphne, was *not* Daphne, but a new planet. M. Goldschmidt accordingly gave the new planet the name of *Pseudo-Daphne.*

M. Luther at Bilk made a careful computation of the orbit of Pseudo-Daphne, and published an ephemeris for the next opposition in December, 1858, but *the planet was not found.*

M. Luther again computed the planet's place for the succeeding opposition in March, 1860; but as, on account of its distance, its brightness should be only one-fourth of its brightness in Sept. 1857, there was not much encouragement to prosecute the search, and the planet was not seen in 1860. He however remarked that at the next opposition in August, 1861, the planet should appear of the 10.11th magnitude, or somewhat brighter than in 1857, and he accordingly published an ephemeris to guide astronomers in their search for it. The planet was discovered by M. Goldschmidt Aug. 27th, 1861,

in R. A. $20^h\ 25^m\ 5^s$ Dec. $-6°\ 48'\ 5''$.

Its place according to the elements of Luther should have been

in R. A. $21^h\ 2^m\ 12^s$ Dec. $-4°\ 49'\cdot 5$.

The observed place therefore differed from the computed place

in R. A. $36^m\ 16^s$ Dec. $2°\ 0'$.

or about ten degrees in arc. This error is not very great, when it is considered that the planet's place was computed from an arc of *less than four degrees, described four years previous.* It is presumed that Pseudo-Daphne will not again escape from the watchful eye of astronomers; but Daphne seems entirely lost, and can only be re-discovered by the same systematic search by which it was discovered in 1856.

3. *Comet* II, 1861. (Continued from p. 165).—Captain Earle of the ship Jireh Swift reports that the comet was seen from the ship in lat. 34° 19′ S. long. 179° 55′ E. on the 18th of May.

On the 25th the tail was 10° long, and on the 26th 15° with a slight curve, the round side being presented to the N.E. Time $5^h\ 10^m$ P. M. lat. 50° 43′ S. long. 163° 8′ W.

G. P. Bond,
Director of the Observatory of Harvard Coll.

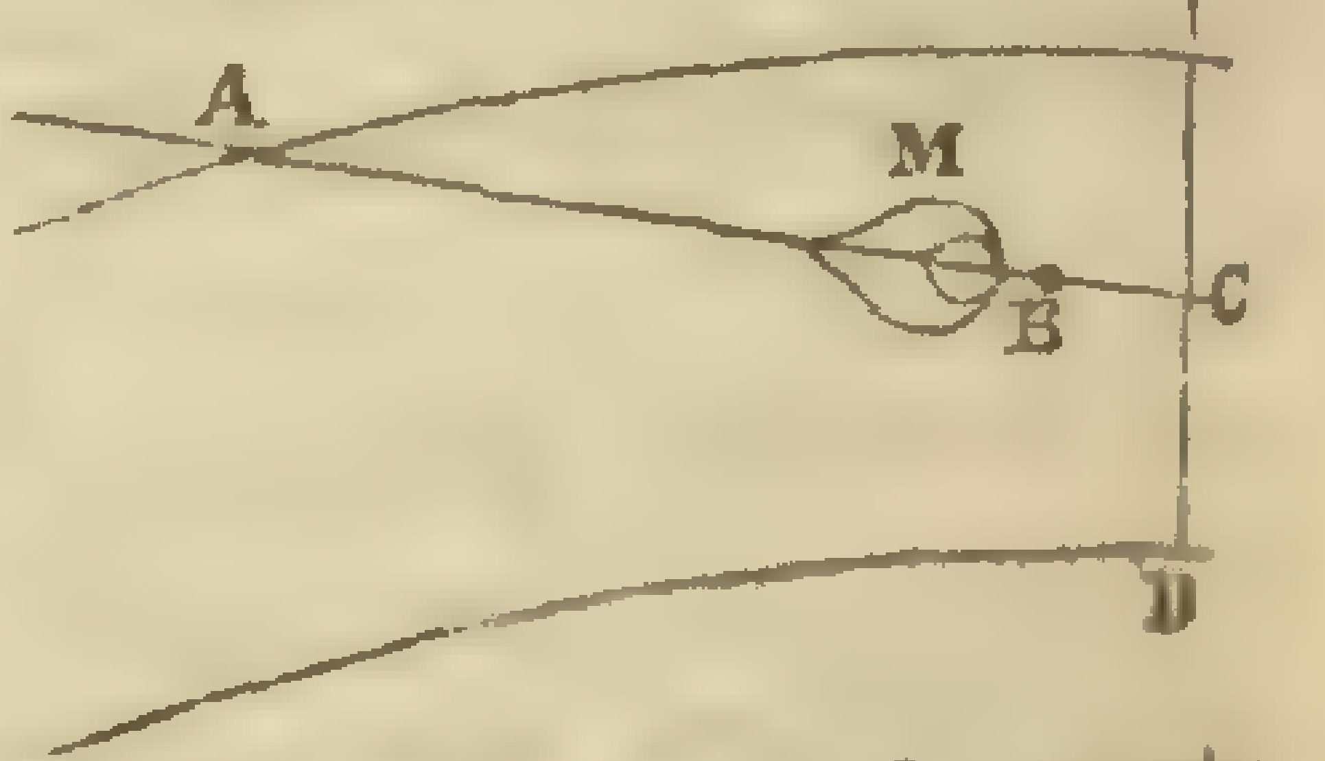
From the Astronomische Nachrichten, No. 1324, we learn that the comet was seen at the Observatory of Sydney, New South Wales, on the 13th of May. On the 27th of May the comet was but just visible to the naked eye. On the 8th of June, it was very conspicuous to the naked eye, being equal to a star of the fourth magnitude. It continued to increase in brilliancy, and on the 21st of June, the tail subtended an angle of about 18°.

METEOROLOGY.—

4. *Notices of Meteoric Masses;* by Director HAIDINGER.—Below we give additional abstracts of Director Haidinger's recent papers on meteoric subjects and intend to publish in the January number of this Journal a more complete account on the nature of meteorites by the same author.

The following additions and explanations to the abstracts already published in this volume, p. 135, are given in a letter of Director Haidinger to Dr. Genth, dated Dornbach, near Vienna, July 31st, 1861.

"The stone of *Pegu* [(5) page 143], is *no other than that* of the fall at Quenggouk, near Bassein in Pegu, on December 27th, 1857, page 142. My excellent friend, Mr. Oldham, Superintendent of the Geological Survey of India, sent the first specimen with only the locality, *Pegu,* because at that time he had not yet received the more accurate notices respecting the fall. But I was not willing to keep back the first account of all the Calcutta meteorites, till I received more detailed information, because I could not depend upon receiving it by a given day, and so I gave it with the year of discovery, 1854, merely by guess. When Mr. Oldham sent the additional notices I corrected my first erroneous statement in the memoir 'on the falls of Quenggouk and Dhurmsala.' But there is still another point relating to the Quenggouk fall and meteorites, very properly adverted to in Prof. Silliman's note, page 139. I had inferred from Mr. Oldham's letter, that the specimen he sent to us, weighing 1 ℔ ¾ loths, was one of the aerolites, quite entire, just in the condition in which it had fallen. This specimen was only partially crusted over. But it now appears I was mistaken in this, as from a later communication I learned that it was only *a fragment* of a larger meteorite, which weighed 4 ℔s 1 oz. 30 grs. Then likely enough, this aerolite was also crusted over the whole surface. As yet I have no answer to a definite question put to him, but I am convinced that the third stone of those which fell, was also crusted all over. The question of a thinner crust covering a part of the surface of aerolites is also adverted to in one of my papers —read July 5th, 1860, II, Einige neuere Nachrichten über Meteoriten, namentlich die von Bokkeveld, New Concord, *Trenzano,* &c., particularly as related to that of Trenzano, page 570, note. If an aerolite should happen to burst at the point C, the end of its cosmic course, it may indeed have a thin crust formed in the very moment of bursting. The slowness of the telluric course or real fall, the downfall, will not present the same probability. But if in the diagram above, the arresting of the meteor should not take place in one single point only, but with several fits or starts as it were and with several reports too, or when either particles by scaling off or a stone bursting perhaps at B, before the whole cosmic course in the atmosphere AC be completed, then there might remain room enough in the space between B and C to have a new and thinner crust formed, and particularly also a sufficient degree of velocity, though already near the end of the cosmic impulse. I find that this paper of mine has never been sent to Prof. Silliman. I enclose it now here and beg you will kindly add it to the collection. * * * *



With the kindest regards and many thanks again, ever most truly yours,

WM. HAIDINGER.

P. S.—I must still add another explanation relative to the movement of the Quenggouk-Pegu meteor. At first it was not quite clear, where the starlike appearance had originated, I mean in what quarter of the compass. "If" it appeared in E.N.E. first and then had *gone* down to the eastward, it must have first come in the direction W.S.W.! Now I wrote again to Mr. Oldham on the subject and had from him the decided reply that the meteor, when seen from the steam-frigate Semiramis, was moving, as nearly as could be remembered, *from W.N.W. to the Eastward.* This would give a course much less violently curved and crooked and indeed much more natural and probable. Mr. Oldham intended to have some additional questions put in order to ascertain the position still more exactly if possible.

This is another very great desideratum in the history of meteorites, the question which quarter of cosmic space they come from, the motion of each of the meteors being conceived to take place in a straight line with a velocity such as is observed or calculated from data, but this is a most difficult subject, though in some cases at least not altogether impossible of solution.

The Parnallee stone of 28th Feb., 1857, came nearly from the constellation of the Fishes;—I gave some notice of that meteorite in our meeting of Academy of the 4th of July; of a new aerolite, or rather one not yet described, in the Government Central Museum at Madras, which fell Jan. 24, 1852 I gave some notice on the 20th of June, as also on the 8th of April and 6th of June, notices of large masses of meteoric iron, found near Melbourne, Victoria, two of them valued at about 1½ ton, one 5 to 6 tons from the Dandenong ranges and near Western-Port. W. H.

1. Meteors resembling that remarkable one, observed on July 20th, 1860,* over a space of more than 1000 miles, from Lake Michigan to Long Island, are of rare occurrence. Notice of an analogous one was given by Dir. W. Haidinger at the meeting of the Imperial Acad. at Vienna of February 7th, 1861.

This was observed by Dr. Gustave Tschermak, immediately after sunset about 7½ o'clock, some evening in August, 1848 or 1849 at Littau, N.W. of Olmütz, in Moravia, as two fireballs or rather distinctly pear-shaped meteors of the greatest brilliancy, the first about three times as large as the area of the moon and larger than the second. He observed them at an altitude of about 11° in W.S.W., moving towards S.W. and disappearing at about 5° above the horizon behind a little house. The phenomenon lasted about 15 seconds; no sound was heard.

Several analogous phenomena are mentioned in Chladni's work, "*Ueber Feuer-meteore.*"

Before the year 1800 only two, and these, not very accurate accounts are recorded, the first of five fireballs, which are said to have fallen a few days after the 30th of November or (according to others) on the 12th of December, 1642, between Gran and Ofen; the second was noticed by the Duc de Guise,† who saw on the 8th of January, 1648, one fire-ball separate into three parts, which afterwards re-united.

Hardly belonging to this class are the two small fireballs, about twice as large as Venus, which were observed in the middle of August, 1800, at 9 P. M. near Halle, moving in a southwesterly direction, because the second appeared five minutes after the first, corresponding in the orbit of the earth to a distance of 6000 miles.

The phenomenon of April 15th, 1804, at 9¾ P. M., shows more analogy with the case of July 20, 1861. Luminous points followed this (1804) meteor moving somewhat downwards from S. to N.; another was observed at Göttingen, Sept. 16th, 1815, after 9 P. M. where eight or ten smaller ones followed a fireball in the same direction.

Still greater similarity was shown by the brilliant fireball of July 17th, 1818, between 9 and 10 P. M., as seen at Montpellier, Vermont.‡ A pear-shaped ball of the size of full moon with its broader end towards the earth, having the appearance of a solid body, and immediately followed by two smaller fireballs.

Arago§ gives in his list a phenomenon, the fireball of Collioure, Dept. of East Pyrenees of February 21st, 1846, consisting of two large luminous balls, almost touching each other and moving from N.E. towards S.W. Also another one* of Aug. 12th, 1850, which according to Coulvier Gravier consisted of small balls.

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* This Jour., [2], vol. xxx, 293.

† Memoires du Duc de Guise, 2d ed., Paris, 1668, p. 323

‡ Journal of Science of Roy. Inst. Lond., No. xi, vol. vi, p. 160.

§ Populäre Astronomie, herausgegeben von Dr. W. G. Hankel, 1859, p. 219.

2. At the same meeting Dir. W. Haidinger gives some details of the fall of the meteoric stones of Parnallee, near Madura in Hindostan, of which the smaller piece is in possession of the Western Reserve College at Hudson, Ohio, containing according to the analysis of Dr. Cassels (this vol., p. 402), 3 per cent of metallic iron and 17 per cent of nickel.

Mr. H. S. Taylor, chief of the American mission at Madura, gave in the transactions of the Geographical Society at Bombay in 1857, the first notice of the fall of two very large stones, one of 37 lbs, the other four times that weight. The larger stone is in the Madras museum, the smaller has been sent to Hudson, Ohio.

According to the report of Mr. Taylor, these stones fell a little S.E. of the village of Parnallee, 9° 14′ N. lat., and 78° 21′ E. of Greenwich, the larger a few seconds before the smaller and two to three miles north of it. From the direction of the hole, which they produced by their fall, they came from about N. 10° W., making with the perpendicular an angle of from 15° to 20°, the small one almost perpendicularly. The most rounded convex part, in which the centre of gravity lay, was lowermost in the earth. The larger mass penetrated the ground two feet five inches, the smaller two feet eight inches; the latter has *not* the appearance of being a fragment of the former.

According to Taylor the smaller stone has a spec. grav. of 3·3, a little higher, if soaked in water. (See Dr. Cassel's results.)

After having been wetted the larger piece showed a crack, which afterwards, perhaps by oxydation, increased in size.

The fall was accompanied by two terrible reports, resembling thunder claps. They were heard at Tuticorin on the coast of the Gulf of Manaar, 40 miles south, and very loud at Madura, 16 miles distant. Although several persons stood in the neighborhood, where the fall took place, neither of the stones was seen falling; a cloud of dust rose from where they struck. Their shape is compared with cannon-balls of large calibre, round, although somewhat irregular, with a black smoky crust, the interior resembling granite with particles of iron.

3. For about seven years it has been known that several huge masses of iron existed near Western-Port, southeast of Melbourne, in Australia, their meteoric origin however, was first suggested by Mr. Fitzgibbon. Several data relating to them obtained from a letter of G. Neumayer, Director of the Flagstaff Observatory to Prof. von Hochstetter, were communicated at the meeting of the Imperial Academy of Vienna of April 18th, 1861, by Director W. Haidinger.

The larger mass weighs from five to six tons, the other about 1½ tons. They are covered with a crust of the well known constitution, in which the usual cavities are not wanting. The relative situation of the two masses is N. 20° E., and they are about three and a half miles apart.

Both masses lie near the surface, so deep that only their tops project above the ground. The tertiary sandstone, occurring at Broughton, is also found there and basalt at the depth of from 12 to 15 feet just as on the coast at Western-Port. The small block lies in 38° 8′, the larger in 38° 11′ S. (about 145° 15′ E. of Greenwich), the first 107 feet, the second 127 feet above tidewater.

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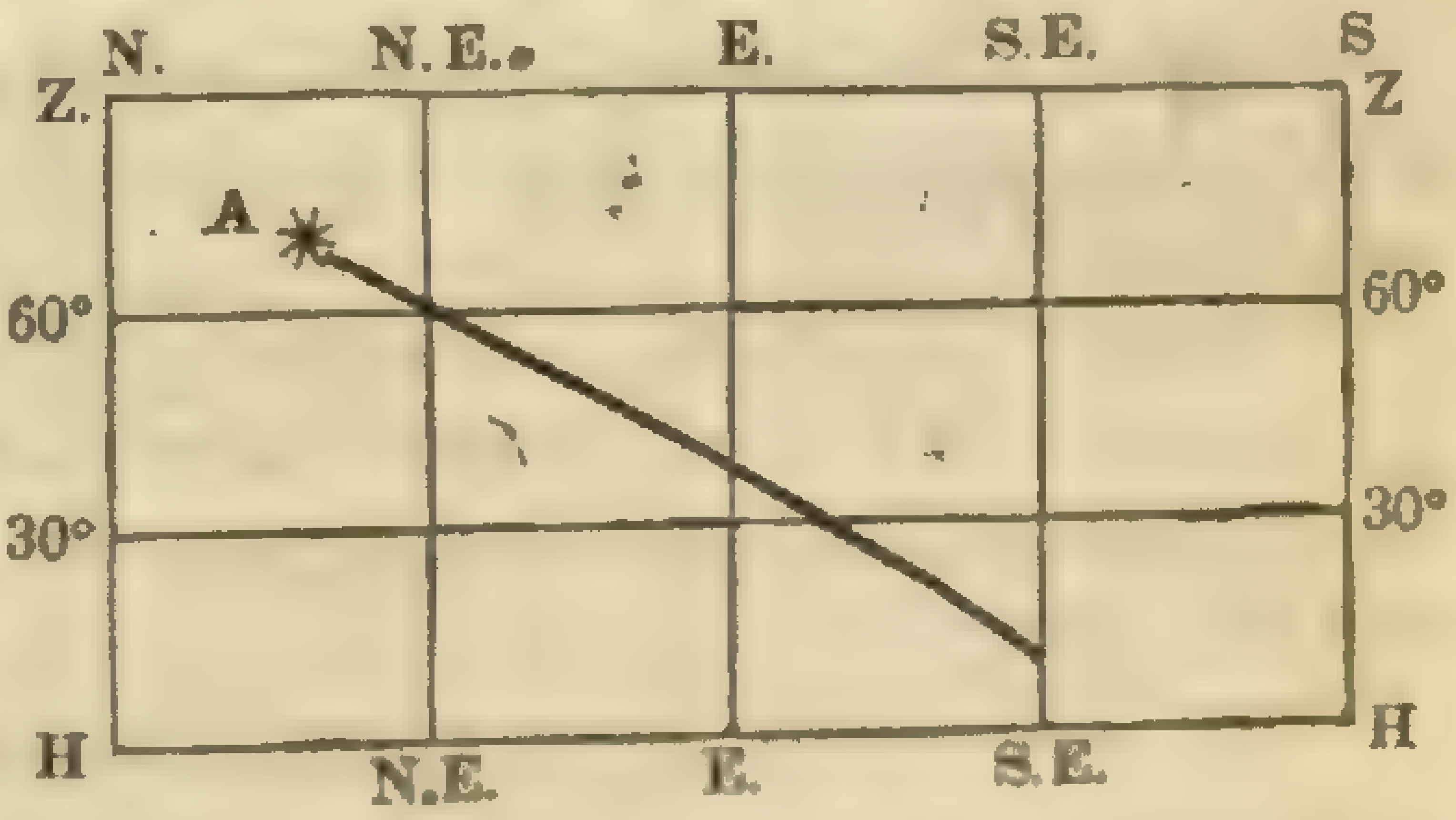
* Loc. cit., p. 224.

The masses showed no polarity, except that resulting from the earth's magnetism. The lower end of both pieces was a strong south pole, the upper a strong north pole; the longitudinal axis of the large piece, which is about five feet long, lies exactly in the magnetic meridian of the locality. It is the intention to bring the smaller mass to Melbourne.

4. On the occasion of the departure of Theodore von Heuglin to his travels in Africa, Dir. W. Haidinger made some suggestions with regard to observations during the journey, of meteors and meteoric stones or irons, whether recent or of former periods.

The observations are to be noted with as much accuracy as possible upon tables, on which hemioramas are prepared after the manner of Mercator's projection. The annexed sketch for example, represents a fireball, A, which appears at N.N.E. at an altitude of 70°, and goes down in a straight line to 10° S.E. The following points are to be taken into consideration.



(1.) The exact geographical situation; the situation as respects the constellations is not necessary, because that always can be found from the geographical position and the time.

(2.) Statement of time—hour, day, month and year.

(3.) Size compared with that of the full moon.

(4.) Form, whether round, pear-shaped, and in what direction, etc.

(5.) It is very important to notice, whether the meteor was first seen as a *star*, constantly increasing in size.

(6.) Duration of the phenomenon.

(7.) Whether it disappears like a star or in its full size, with statements of any additional circumstances.

(8.) Colors.

(9.) Phenomena of sound.

(10.) Circumstances connected with the fall itself.

(11.) Whether the material consists of iron or stone or any other substance.

(12.) Whether the fallen body is glowing or hot, or warm, perhaps warm on the surface, whilst cold inside.

It is of the greatest importance to collect and send on such substances; but since iron-masses are often very large and heavy, fragments ought to be taken, with careful drawings and measurements of the whole mass.

F. A. Gth.

5. *Remarkable Meteor, Oct.* 4, 1861.—On Friday, Oct. 4, 1861, at 7h 20m p. m., a brilliant meteor was seen at New Haven, shooting overhead from W. to E., and leaving a long bright track, from Cygnus to Cassiopeia, which was visible at least 120 seconds, slowly assuming a serpentine form. It was also seen at Litchfield, Conn., with like appearances.

[Remark.—The following paper by Prof. Twining was received too late to find its proper place among the original memoirs. But as it contains suggestions of much importance applicable to the succeeding return of the November meteoric shower, it seems proper to publish it without delay.—*Eds.*]

6. *Observations respecting the Periodic Meteors of August;* by ALEX. C. TWINING.

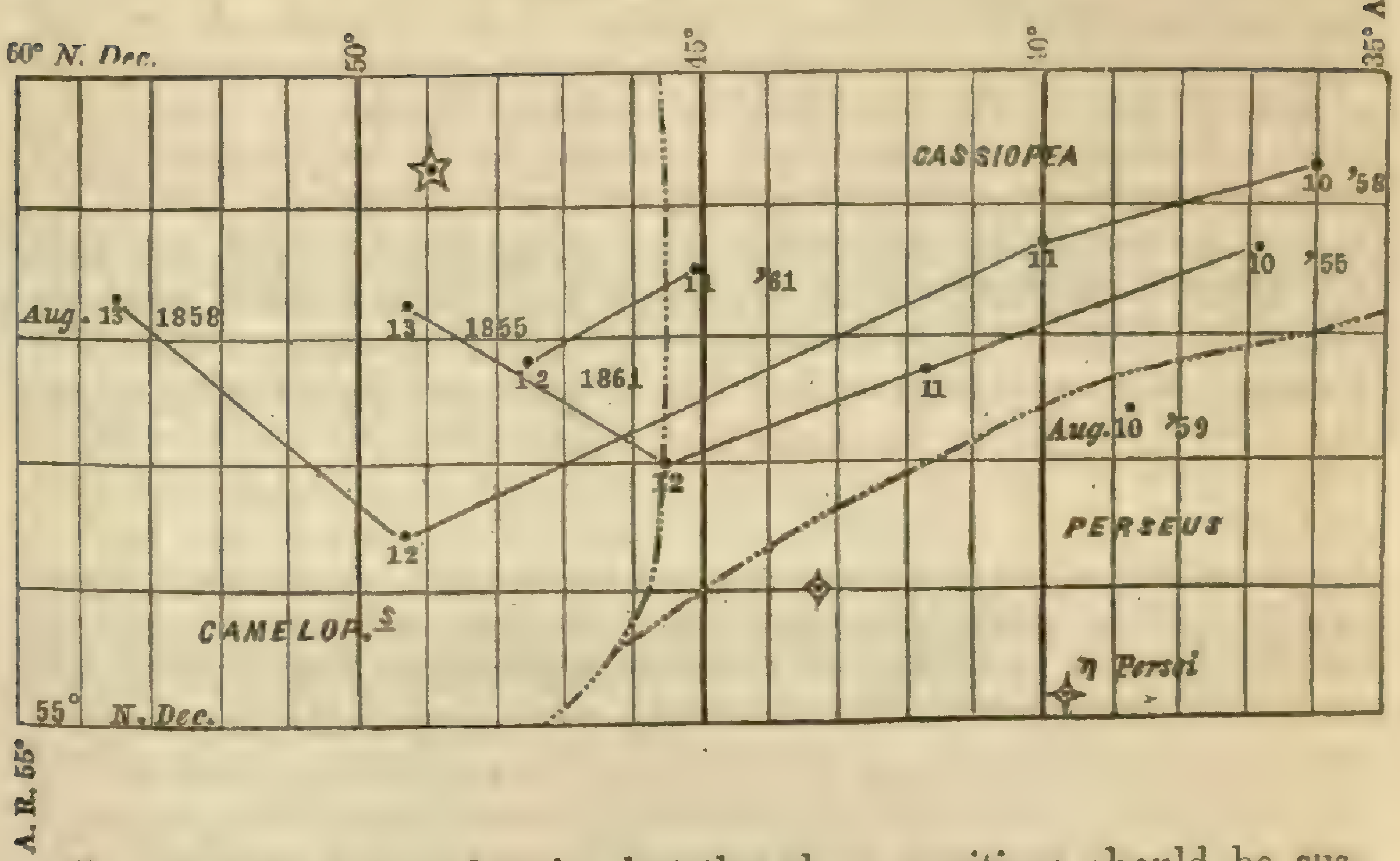
Upon every occurrence of the August meteors, which I have noticed with critical attention, my observations have been directed especially to the position of the "Radiant." Of the conclusions arrived at there are two of special interest :—

First; The position of the radiant is probably capable of a far more exact determination than is ordinarily supposed, or than could have been anticipated.

Second; The radiant is apparently subject to a motion of several degrees from day to day, and one which exhibits some remarkable points of agreement in the comparison of one year's positions with those of other years.

The accompanying fragmentary plane chart will illustrate these conclusions in detail. Upon it are projected lines of declination and right ascension, with the position of a few stars—all taken graphically from the North Polar Star Chart of the British Society for the Diffusion of Useful Knowledge. My observed positions of the radiant are also given by a minute circular disc, together with the year and the day of the month for each position. I have also distinguished the positions of each year considered as a series or group by tracing a fine line consecutively from position to position. By this grouping of the observations into distinct series, the eye will at once discern the correspondence, or want of correspondence, in the position and motions of the several years. It should be observed that the positions were determined in different localities in different years. The observations of 1855 were made at New Haven, those of 1858 at Cleveland, Ohio; that of 1859 at Boston, and those of 1861 again at New Haven. Whether this variation of localities has varied the groups or series compared with the same as they would have been seen from one and the same point of observation, is a question which I am inclined to answer in the negative, but not with entire confidence. Undoubtedly parallels and meridians and the time of day will affect the radiant positions unless the primary relative velocity of the meteors and the earth is so great as to make that deviation of orbit which is consequent upon the earth's attraction not a large one. The times of day were not always the same for the positions in the chart; but they were always within either about half an hour earlier or three hours later than the initial instant of the morning whose date is applied to the position. Thus a position taken on the 10th may have been as early as 11½ P. M. of the 9th or as late as 3 A. M. of the 10th,—more frequently the latter than the former.

Chart of Positions.



I confess to an apprehension lest the above positions should be suspected of having undergone some sort of empirical accommodation to bring them into correspondence. It is a fact however that my memory always dropped the results as soon as recorded, and the record was not at hand for consultation from year to year. Moreover, some of the series exhibit erratic changes which neither judgment nor imagination could be tempted to originate. I remark, in passing, that the positions were always determined by employing the best accessible star charts, and, although the charts were modern, their epoch may have varied among themselves a little—say not to exceed half a degree of position.

It is very clear that many of the meteors which are properly marked "conformable" do not describe a flight that can be traced back exactly to the radiant. It is also true that the "non-conformable" meteors often appear, in small numbers—three to five—to have a common point of divergence in some quite distant locality of the heavens. What then —it may be asked—*is* this "radiant" whose position can be marked so definitely. I reply—it is not a point of exact apparent divergence of all the conformable meteors, but it is such a point for the great mass or assemblage of them, so far as one observer can judge; and it is also, under the same visual limitation, the centre of that area within which the others would project back their lines or directions. The process by which it is found has been, in my own case, the following:—The first few meteors—say five to seven—determine the locality rudely. Fixing our attention primarily upon this locality we are soon supplied with some contiguous and very definite flights which cut the area in a line that can be traced and kept in mind. It may be that this line will be shifted laterally by other nearly parallel flights. We next look with interest for other flights crossing the first at right angles or at a large angle; and when a few such are obtained they limit our area to a narrow central space subject only to slight shiftings one way or another according to judgment continually and rigidly exercised in view of the successive flights which can be brought into comparison with it. These flights should not ordinarily be very distant from the central area. In fact the very *abbreviated* streaks almost in immediate proximity to the radiant are of special value in the determination or the verification. Occasionally, indeed, a meteor will show itself stationary in the very radiant itself. In the year 1855 on the night of Aug. 10th such a stationary meteor appeared and almost instantly disappeared in the spot to which my sight was directed—with a peculiar effect, as if a new fixed star had suddenly begun its existence and as suddenly ended it. Not many minutes after a second came and went with a like singular and bewildering effect; and while I was in the act of pointing out its exact position to a fellow observer (Mr. Francis Bradley) a third drew from us both a simultaneous exclamation. This remarkable consecution of a phenomenon against which there were so many chances, and against the repetition of which in such a brief space the chances were so many millions of millions, satisfied me that multitudes of undiscovered meteors were in play over us, but for some reason not seen except when their flight was directed exactly to the observer. If this was the fact a telescope *truly pointed to the radiant* would have discovered yet more stationary points of brightness. Such an attempt if successfully made and ascertained to be ordinarily practicable, would realize this remarkable advantage—that, by employing the telescopes of graduated instruments, *our radiant positions might be defined with certainty, and with some rude approach, at least, to astronomical accuracy.* For although varieties of direction obtain, yet there may be a preponderating number so nearly the same as to exhibit a point of great and sudden concentration—like the image of a star by a chromatic lens. I am not aware that any of the few telescopic researches hitherto made have been directed to such a study of the radiant.

The simple method above explained appears to me more accurate for a determination of the radiant, than it would be to map down every flight in course as it occurs,—because of the unavoidable errors in estimating arcual distances from the stars of reference—a very serious error in the direction of moderate flights, but one which does not at all enter into the directions traced back by the mind at the moment of the flight. But, for other purposes, the mapping is indispensable to completeness of observation and record.

For any considerable advance upon our present knowledge of meteors and meteoric rings, we are clearly dependent upon accurate, systematic and *concerted* observations. The enumeration of flights both conformable and unconformable, is very important;* but the three elements of *radiant position*, *changes in its position*, and of the *meteors' velocity*, are obviously of elementary and primary importance, because those determine the plane, the figure and the magnitude of the ring. Even a casual observation of the principal meteor of the last periodic display, made coincidently—although without concert—by Mr. E. C. Herrick and the writer at New Haven, and by Mr. Benj. V. Marsh at Burlington, New Jersey, has seemed decisive, upon even the rudest attention, that the meteors of November and the meteors of August are independent and distinct in their origin or source. Still beyond this, an accurate discussion of the observations may even develop and determine within near limits the *actual conditions of the great meteoric ring of August.* But this need not be pursued here; for, happily, this discussion has been undertaken by Prof. H. A. Newton of Yale College, whose results, if they appear, will doubtless test conclusively the correctness of these opinions. But the four elements above referred to—including numbers—do not exhaust the *desiderata;* among which may here be instanced the absolute height of the upper and lower extremities of the meteors' paths,—their specific phenomena of combustion and dissipation,—and their irregularities, intermissions, or rebounds, as well as explosions.

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* My own enumeration for August last, having been referred to among those reported by Mr. Herrick, in the last number of this Journal, are here given. The

The circumstance that upon certain definite days in August of each year, and in November of many years, observers will surely be rewarded with abundant opportunities and subjects for their attention is, of itself, one inestimable encouragement to concert and assiduity. The writer has already ventured the opinion in this Journal,* that, beyond a definite limit of the earth's atmosphere proper, there exists a secondary or external atmosphere—possibly of aqueous vapor,—that in this external medium the shooting stars become visible,—and that a knowledge of its upper limit may be obtained by considering and comparing the upper limit of the meteors' paths. It has long been his *suspicion,* to say the least, that some of the irregularities or specialities in meteor's flights above referred to are to be explained by their encountering a sudden change of medium from a secondary or exterior atmosphere to the atmosphere proper.† But to

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times are those of the place of observation, i. e., New Haven, Conn., the locality circular with a radius of 50° around the Radiant.

Saturday, Aug. 10th, 1861, $10^h\ 30^m$ P. M. began observing.

From $10^h\ 30^m$ to $10^h\ 40^m$, for this interval of time, one a minute.
“ “ “ $11^h\ 0$ 12 conformable, 4 unconformable.
“ “ “ $12^h\ 0$ 41 “ 10 “
“ “ Aug. 11th, 1^h, 78 “ 17 “ total, 95 in $2^h\ 30^m$.

Sunday, Aug. 11, at $9^h\ 30^m$ P. M., began observing.

From $9^h\ 30^m$ to $10^h\ 0^m$, 4 conformable, 2 uncon.
“ “ “ $11^h\ 0^m$ 17 “ 7 “
“ “ “ $12^h\ 0^m$ 36 “ 11 “ total, 47 in $2^h\ 30^m$.

On Saturday as many as five showed curved or irregular flights. The 29th, near the zenith, surpassed all others in brilliance, and in the remarkable extent and duration of its train, which was visible one third of a minute or more, and faded gradually from the ends to the middle. Time of flight 1·2 seconds.

* Vol. xxvi, note to p. 20.

† I may be excused for here propounding the enquiry, which has been forced upon me by certain auroral phenomena, whether some of those phenomena,—clouds, arches and streamers—have not commonly their lower limit at the surface of the atmosphere proper and their upper at a secondary atmospheric limit, as above suggested. So long ago as 1835 this was forcibly suggested and made almost *visual* by the flat base, and horizontal motion exactly along its base, of the singular auroral cloud described by me in vol. xxxii [1], p. 217–219 of this Journal. Some auroral arches also by their first dense formation and fast motions diminishing in both respects and suddenly dissolving when almost stationary, confer a plausibility upon the conjecture that they are formed at a definite lower surface and expand or rise till they are dissipated at a definite higher limit.

realize the best attainable completeness of observation and record, certain preparations and facilities are doubtless important,—for example,

First; The ability to estimate arcs correctly in their angular amount,—especially small arcs.

Second; Readiness in measuring minute intervals of time by a mental standard closely accurate to tenths of a second.*

Third; Familiarity with all stars of the first five magnitudes in the specific area over which the observer maintains his watch.

Returning for a moment to the above chart, it need only be added that the radiant positions indicated thereon, as well as the other topics above referred to, are brought forward in the expectation of eliciting like observations from others, and in the hope of somewhat exciting a new zeal and assiduity, especially with reference to the coming November meteors.

Note by the Editors.—The Connecticut Academy of Arts and Sciences have lately instituted a Committee (of which Prof. Twining is chairman) to invite coöperation in the observation of the November and August meteors. The Editors take this occasion to say that observations duly recorded and forwarded to the Academy will doubtless receive a merited attention and announcement. They should be addressed to the chairman.

7. *Grand Meteor of August* 10, 1861.—*The August ring of Meteors;* by H. A. Newton.—The observations of this meteor (pp. 294–5 of this volume) give interesting results. It was seen by Mr. Marsh at Burlington, N. J., (lat. N. 40° 5′, lon. W. 74° 52′ 40″), and by Prof. Twining and Mr. Herrick, at New Haven, (lat. N. 41° 18′, lon. W. 72° 55′ 25″).

Mr. Marsh says, "I was looking exactly in the direction of the meteor and had a perfectly satisfactory view of it, and during the visibility of the train (20 sec.) noted with all possible care its position among the stars. I concluded that the track was very nearly in the line of τ Persei and θ Persei, and the point of disappearance was about in a straight line joining β Persei and γ Andromedæ,—the length of the track being about equal to the distance between η and τ Persei. I continued my inspection of the region some time after the disappearance of the meteor in order to fix the track firmly in my mind. The meteor itself lasted only one or two seconds. The time was about 11^h 23^m P. M."

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* I recommend for a standard the repetition of a selected sentence inaudibly, without motion of the organs except the tongue. The syllables are to mark tenths of a second each, and should be of equal ease in pronouncing, and either nine in number or nineteen accordingly as you measure by seconds or by double seconds. The initial instant of the phenomenon to be timed, forms the *epoch* or zero of measurement, and the syllables are to be repeated at a uniform rate acquired by practice with a seconds pendulum or other index. The last tenth of the second, or double second, is marked by enunciating "one" (called "un" to avoid hiatus),—in like manner the last tenth or end of the second, third, fourth, &c., second or double second is marked by "two," "three," &c., on to "ten." Thus we shall have measured ten seconds, or better, ten double seconds or twenty seconds.

A brief practice will evince to any one the accuracy with which the mind forms a conception of time, and a long and intermitted practice the persistence with which it retains the standard thus distinctly formed, even after disuse.

Again, in the instance of these brief and *unannounced* flights most observers will best apply their standard, not to the phenomenon itself, but, on the instant after, to the *conception* of its duration which is then vivid in the mind.

Prof. Twining says, "The meteor described by Mr. B. V. Marsh as the most brilliant of the display is identified by myself by several coincidences.

1st. It was preëminently the most brilliant flight within my time of observation.

2d. The time at New Haven was a very few minutes before 11½ P. M.

3d. The duration of the train was timed by myself afterward from recollection, and was less than half a minute, and more than a quarter.

The following are my own observations of the meteor's flight:

1st. Its line was traced back critically to the radiant which I find I had marked on the star chart of the Society for the diffusion of Useful Knowledge at R. A. 45°, N. P. D. 31° 30′.

2d. The flight was timed carefully and was found 1·2 seconds. This may be relied upon as a near approximation, at least.

3d. The meteor began at a point distant about 64° of arc from my radiant. It passed within a few degrees of the Dolphin. Its beginning was—say a degree—S.W. of a star which probably was ε Cygni. I did not record the position laterally, that is to say, from E. to W., but the above distance remains unaffected by that circumstance.

4th. Estimated by subsequent examinations of the heavens I call its length of flight 29°."

Mr. Herrick says, "While we were intently watching the meteors of August 10, 1861, each of the four observers looking at an altitude of about 60°, we were all startled, about half past eleven P. M., by a very brilliant flash. Instantly turning for the cause, we saw among the stars overhead a bright phosphoric bar, the meteor having already vanished. This bar, which I locate nearly as stated by Prof. Twining, remained visible without material change of place or shape about 20 seconds, by estimate. From all the circumstances of the case I infer that the time of the meteor's flight could not have exceeded one second."

These observations indicate a visible track of about 33 statute miles, with altitudes above the earth's surface of about 70, and 54, miles at the beginning and ending. Its course was conformable, and there is every reason to suppose that it was one of the August group of meteors.

If the time of flight is 1·2 seconds, the velocity is 27·5 miles a second. The resistance of the atmosphere is wholly unknown, and for it no definite allowance can be made. For the increase due to the earth's attraction, subtract 48 from the square of 27·5 and take the square root of the remainder, agreeable to the principle of conservation of living forces. Though not an exact process this gives a tolerable correction where the velocity is large. The result is 26·6 miles a second for the relative velocity of the meteor.

The well established fact that the meteors of August 9–11 move in paths which produced backward pass through a small region of the heavens, and that this region of emanation remains the same, or nearly the same, from year to year, implies:

1st. That the individual meteors are cosmical bodies;

2d. That they are permanent members of the solar system revolving about the sun in *elliptic* orbits;

3d. That the direction and velocity of the relative motion, and therefore of the absolute motion, of the individual bodies are nearly the same;

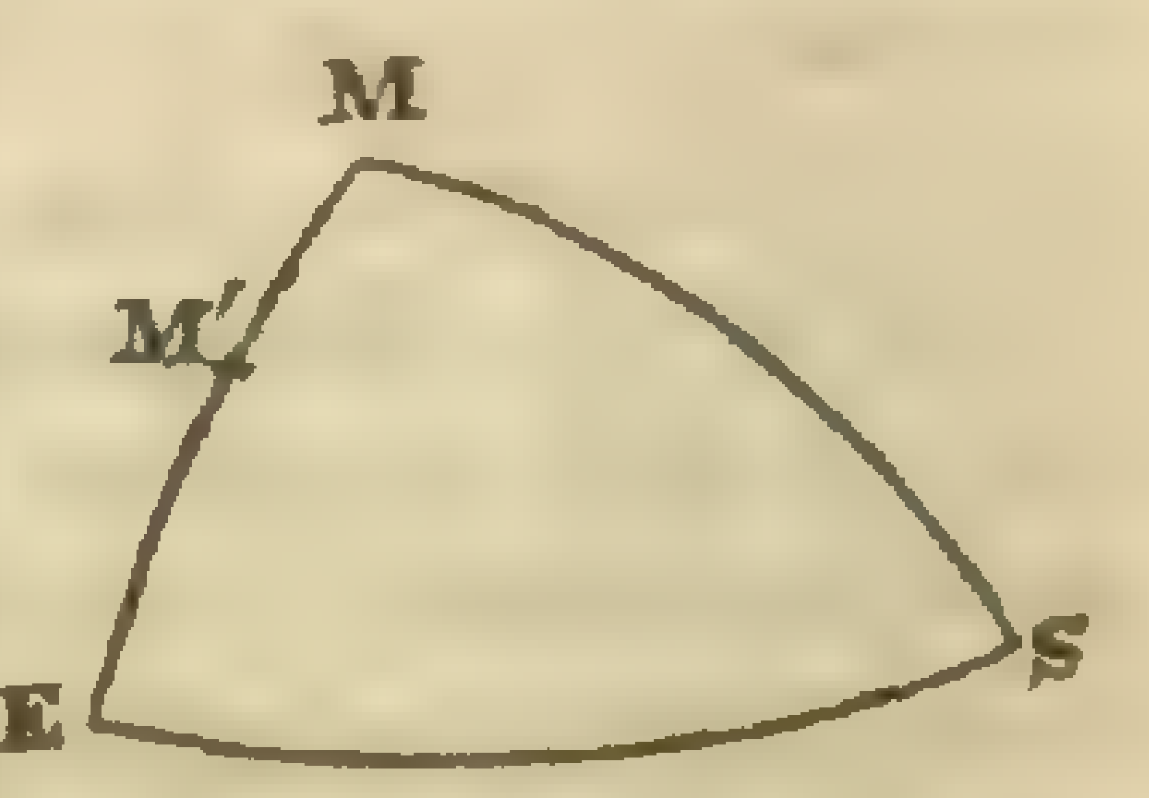
4th. That the whole group form what may be considered a ring, or disk, around the sun.

The region of emanation has not great length in the great circle through it and that point of the heavens to which the earth is moving. Hence the velocities of the individual meteors, of the same year, and of different years, are nearly the same.

The region of emanation has not great length in the great circle through it and the sun. Hence the tangents to the individual orbits make nearly the same angles with the sun's radius vector.

It follows that the elements of the individual orbits are nearly the same, and hence the probable conclusion that the meteors form a ring, and not a disk of great breadth in its own plane. If the breadth is very large, we must conclude that the great part of those orbits which cut the earth's orbit belong to a limited zone, or ring, in the disk. The mean velocity and direction of motion of the August meteors would give an elliptic orbit about the sun which would represent the ring. The velocity of a single member of the group and the mean place of the radiant, give approximate data for determining this orbit.

Let EMS be a triangle on the celestial sphere, E the point to which the earth is moving, S the sun's place, M′ the mean place of the radiant, and M the point from which the meteors are moving in their orbit around the sun. Since the motion from M′ is the resultant of the motions of the earth and the meteors, E, M′, and M, are in the same great circle. The place of M′ is found by observation, and if the mean value be taken there need be no correction for the earth's attraction. From the places of M′, and E, can be computed the angle E, and the arc EM′. ES can be called a quadrant.



Let v, v', and v'', represent the earth's velocity, the true, and the relative velocities of the meteors, v' and v'' being corrected for the earth's attraction. By the law of composition of motions $v'^2 = v^2 + v''^2 - 2vv'' \cos EM'$, and $v' \sin EM = v'' \sin EM'$. Also in the triangle ESM, $\cos SM = \sin EM \cos E$, and $\tan S = \sin E \tan EM$. But S (or $\pi - S$) is the inclination of the ring to the ecliptic, and SM the angle which its tangent makes with the sun's radius vector. These with the value of v' give the elements of the ring.

Assuming M′ to be at R. A. 42°, N. P. D. 34°, E to be in the ecliptic at R. A. 46° 30′, v'' as above equal to 26·6, and v (including the earth's rotation) equal to 18·9 miles a second, we find,

EM′ = 39°, v' = 16·8, SM = 78°.
SEM = 78°, EM = 84°, ESM = 84°.

These give for the ring's semimajor axis 0·84, *for its ellipticity* 0·28, *its perihelion distance* 0·60, *its inclination* 96°, *and the periodic time* 281 *days.* The ellipticity cannot be less than cos SM, which is near 0·20.

The inclination of the ring to the ecliptic must be greater than 60°. For the greatest value of v' is 26·7 miles a second, that which belongs to a parabolic orbit. This with the radiant above assumed makes ESM > 65°. That ESM be less than 60°, the dec. of M′ must be reduced to 52° 40′, which appears hardly admissible. On the other hand to give ESM a value greater than 120° would require that v'' be less than 16·7, or the observed velocity less than 18 miles per second. The observations of Messrs.

Herrick, Twining and Marsh, seem to render this also inadmissible. Until other determinations of velocity are made, 96°, as determined above, or 84° with retrograde motion, must be considered the most probable inclination. In any case *the meteors of the other known annual displays cannot proceed from this ring.*

The thickness of the ring is 5 to 10 millions of miles, for the earth, moving nearly two millions of miles a day, is immersed in it during several days. Some idea of its breadth could probably be obtained by observing the area of the region of emanation, or in other words, the average length of the perpendiculars from the centre of this area upon the paths, produced backward. Observations in the southern hemisphere from the 4th to the 8th of February are desirable, to determine whether the earth's orbit cuts the ring, or disk, at the other node.

A rude estimate of the number of individuals in the ring may be formed. Several observers in one place in the morning hours of Aug. 10–11 see at least 150 meteors, of which over three-fourths are conformable. Assume the average *perpendicular* distance of the paths of visible meteors from the observers to be not greater than 71 miles. This implies that not less than 112 meteors pass through a circle of 100 miles radius, the circle being at right angles to the relative motion. The velocity is so great that the earth's attraction is not of much account in making the number seen greater than the average throughout the ring. Reducing 112 by the ratio $v':v''$, and calling the cross section of the stream not less than the area of a circle whose radius is 2,500,000 miles, we have at least $(2{,}500{,}000)^2 \times 112\, v' \div (100)^2 v''$ meteors passing the node per hour. In 281 days, the periodic time, we have more than 300,000,000,000,000 bodies for the whole August ring.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Return of the Arctic Expedition of Dr. Hayes.*—Dr. Hayes arrived at Halifax, N. S., on the 9th of October (and arrived at Boston, October 23d, 1861), with important additions to Arctic Geography, but disappointed as to the chief object of the Expedition. Science has to mourn the loss of the astronomer, August Sontag; the carpenter Gibson Caruthers also died during the voyage. Waiting the more exact account of his voyage which we look for from Dr. Hayes we quote meantime from the public journals the following notice of the voyage and its results.

The United States sailed from Boston July 10th, 1860. She reached Upernavik after a short but stormy passage and proceeded thence to Smith's Straits, where she remained until July 10, 1861.

On the 14th of April, however, a party left the vessel, consisting of thirteen men and sixteen dogs, with boats on sledges. The leader of this dog-train is now on board the United States, and is a fine specimen of the species. The party reached lat. 79° in the Middle Smith's Straits, and here the party divided—Dr. Hayes and three others went as far as lat. 81° 35′, west side of Kennedy Channel, (forty miles further northward than the latitude attained by Dr. Kane in 1854,) and were there obliged to put back, their provisions being exhausted. Dr. Hayes reached his vessel on the 27th of May. On the 13th of July, 1861, they proceeded to Littleton Island, where the vessel remained until the 27th. Sailing thence in a northerly direction they were met by immense packs of ice, which the vessel could not penetrate. She then made for Cape Isabella, on the west side of Smith's Straits, which was safely reached. Boat parties were sent out from here to explore, but the ice was so solid that no chance was found for proceeding. The United States next came to anchor at the Esquimaux settlement of Natik, Northumberland Island, on the coast of Greenland.

After surveying Whale Sound, the vessel sailed for Upernavik, on the voyage home, arriving there on the 15th August, 1861, after passing through one hundred and fifty miles of field ice in Melville Bay. She sailed thence for Discoe Island, and on the 17th September left for New York. From September 24 to October 7 the United States experienced very heavy gales, and sustained some slight injury to her sails and gear.

August Sontag, the astronomer, a gentleman of high scientific attainments, who accompanied Dr. Kane on his last expedition, and was at one time connected with the United States Coast Survey, was frozen to death in his sledge while out exploring, accompanied by a single Esquimaux. He had unfortunately driven upon thin ice which gave way, completely wetting him, and before he could reach a shelter was past recovery. The body was recovered and interred at Port Folke, near Cape Alexander.

The carpenter, Gibson Caruthers, died during the voyage. These are the only deaths out of the crew of sixteen persons which left Boston. The party was unusually free from sickness.

Six men belonging to the wrecked whaleship St. Andrew of Aberdeen, joined the United States at Discoe, making the crew twenty in number.

Hans, an Esquimaux, on whom Dr. Kane placed great dependence, who is frequently mentioned in Dr. Kane's book, and who deserted that expedition while in the ice in the far north, was found at Cape York by the crew of the United States, and returned in the vessel to Upernavik, whence he had started with Dr. Kane.

The expedition went as far north as 81° 35′, a latitude which is said to have been before reached only by Parry in 1827–8. On the coldest day experienced the thermometer fell to 68° below zero.

The vessel was provisioned for two years. The following is a correct list of those of the officers and crew of the U. S. who returned in the vessel:

Commander, I. I. Hayes, M. D.; Captain, S. J. McCormick; first mate, H. W. Dodge; second mate, John McDonald; commander's secretary, G. F. Khoor; assistant astronomer, H. D. Radcliffs; master's mate, Colin C. Starr; a cook, steward, cabin boy, and ten men before the mast.

At a dinner given to Dr. Hayes by the Medical Society at Halifax he made the following remarks:

"You have intimated to me, Mr. President, that a sketch of our voyage would be acceptable to the gentlemen who honor me with their courteous attention. We visited Smith Strait on the 26th of August of last year. Heavy ice and stormy gales prevented our penetrating far within the Strait; and after being twice in jeopardy among the bergs, and three times driven out of the Strait by northeast gales we were forced to go into winter quarters on the east side of the Strait, in latitude 78° 17′ S. I expected to have reached the west coast, and to have secured a harbor near latitude 80°. My plans of exploration were dependent upon dogs, of which an ample stock had been obtained in Southern Greenland. Most of these animals died during the winter, and I was obliged to take the field last spring with a weak force, and in an unfavorable position. I carried with me a boat mounted upon runners for service in the open sea to the northward. After a trial of nearly a month it was found that the boat could not be transported across the Strait, and I accordingly sent it back, and with three companions and two sledges drawn by dogs, I continued northward. On the 18th of May our provisions were exhausted and we returned, having reached latitude 81° 35′; a degree of Northing which I believe not to have been exceeded by any other person except Sir Edward Parry. The land which we explored is the nearest to the North Pole of any which is known. Beyond that land I believe there exists a perpetual open sea, which may be navigated. For this purpose, however, steam power is necessary.

It is my purpose to renew the attempt next year, if circumstances prove favorable; and I am still of the opinion that with steam power, a strong force of men and dogs, and a well organized system of advance depots, the North Pole can be reached. That the region about the Pole should be explored, you will I think all agree. It has long enough remained a terra incognita. Speaking as one interested in the advancement of science, I may say that I care not under what flag the enterprise may be conducted, whether that of America, or England, or France, Science will claim the honor of the achievement."

Dr. Hayes has succeeded in obtaining materials as extensive as could possibly be collected under the adverse circumstances of his voyage. Since the death of Mr. Sontag, the entire responsibility of pursuing the scientific experiments of the expedition has devolved upon Dr. Hayes, who has been a hard worker; and the results at which he has arrived show with what industry his investigations in the various departments of science have been pursued.

The chief results of the expedition are as follows: the completion of the survey of Smith's Sound; the discovery of a new channel at the westward of Smith's Strait; the confirmation of Dr. Kane's theory respecting an open polar sea; the determination of the magnetic dip, and of the declination at many points within the arctic circle; surveys of glaciers, by which their rate of movement is determined; pendulum experiments, and hydrographic surveys; a continuous set of meteorological observations; a large collection of specimens of natural history; a valuable collection of geological specimens; the accomplishment of a higher north latitude than ever before attained upon land; and lastly a large collection of photographic views of the country, icebergs and settlements of the natives.

The labors of the expedition have certainly not been in vain, and Dr. Hayes and his gallant companions are deserving of a generous welcome at the hands of the friends of science, and of Arctic exploration.

2. *The earthquake of* 31*st August.*—This earthquake was felt at the Washington Observatory at 5^{h} 22^{m} A. M. There were two marked shocks, each a succession of long waves of slight elevation, apparently proceeding from south to north. The interval between the shocks was perhaps five seconds. They were accompanied by the usual rumbling noise and were sufficiently severe to rock the water-ewer in its basin and jar the furniture of the chamber. The sound of the earthwave through the air continued audible some seconds after the tremor had ceased. To one who had

experienced several hundred analogous phenomena of nearly every degree of violence, that of Saturday morning, Aug. 31, was unmistakable. J. M. G.

3. *Letter from C. Hitchcock, Esq., on the first observation on the fossils of the Red Sandstone formation of Vermont.*—Eds. Silliman's Journal: As a notice of the Conocephalites from the Red Sandrock series in Highgate, Vt., has appeared in your Journal, (Second Series, vol. xxxii, p. 232), it is but just to the dead to state who were the original discoverers of this trilobite. By referring to the Third Annual Rept. Geol. Vt., 1847, pages 14 and 31, it will appear that Prof. Z. Thompson conducted Prof. C. B. Adams to Highgate, where both gentlemen procured a large number of these trilobites. They were sent to Prof. J. Hall in 1847 for determination who gave them the name Conocephalus, the same genus to which Mr. Billings now refers them. At that time the precise position of the Conocephalus was not known. Nor was Prof. Hall able to give more definite information respecting them in 1858 when I showed him the specimens again.

These trilobites are noticed on pages 339 and 340 of our Third Report on the Geology of Vermont, which will be ready shortly for distribution.

Amherst, Mass., Oct. 23d, 1861.

[For Prof. Adams's own notice of the Champlain Division of the Taconic Rocks, see this Journal, [2], v, 109. We understand from Mr. Hunt that Mr. Billings has, since the date of his paper, here quoted by Mr. Hitchcock, seen Prof. Adams's remarks before the American Association, &c., at Boston in 1847, and quotes them in a paper now in press, indeed Mr. Billings has said the same to us in a late letter.—Eds.]

4. *Atlantic and Pacific Telegraph.*—The 28th of October witnessed the successful completion of the telegraphic connection of the Atlantic and Pacific Oceans by way of New York and Saint Louis to San Francisco and California. It is proposed to extend this line of communication to the mouth of the Amoor River, crossing Behrings Straits by a cable forty miles in length, and thus by way of the Russian lines to Central and Western Europe. About 1700 miles of wire it is said will accomplish this connection. We can then hope to receive our European and Asiatic news by way of the Pacific!

5. *Nicotine.*—It is stated that the tobacco crop of the world is 250 millions of kilogrammes (= 5,512,500 lbs. av.). Schlosing, as already quoted, this Journal, [1], iv, 273, found in various tobaccos an average of about 5 per cent of nicotine.

It is clear therefore that about twelve and a half millions of kilogrammes (=2,756,250 lbs.) of this poison are annually produced. As the sp. gr. of nicotine very slightly exceeds that of water, this quantity would fill nearly 100,000 wine barrels, and would give twelve and a half grammes (= 293·025 grains) to every man, woman and child on the globe. As a few drops will produce death, it is probably much within the mark to say, that one year's crop of nicotine could destroy every living creature on the face of the globe if its proportion was administered in a single dose.

6. *Geological Survey of Wisconsin.*—The report of the Survey of this State is in press under the supervision of Prof. James Hall. This volume, of 500 pages or more, will contain a chapter on the general geology,

with a map, and full illustrations of the characteristic fossils of the several rock formations; also the elaborate report of Prof. J. D. Whitney, (now at the head of the geological survey of California,) on the lead region, with an enlarged sectional map showing the location of the mines, &c. Mr. T. J. Hale has been engaged during the past season making explorations under the direction of Prof. Hall, chiefly in the Northwestern counties.

The specimen of the plates of fossils which we have seen is very creditably executed in the best lithographic style.

7. *Eighty Years Progress of the United States, showing the various channels of Industry and Education, &c.* 2 vols. 8vo. pp. 457 and 455. New York, L. Stebbins, 51 John st. Compiled "by eminent Literary men."—This compendium of national statistics forms a valuable hand-book of reference, to which all who possess it will have frequent occasion to turn for information in respect to the progress and condition of the great elements of growth and development in the history of the United States during eighty years past. The value of the book as a work of reference would have been much enhanced by a more frequent reference to authorities and original sources of information. But taken as it is, it supplies a great desideratum, and its pains-taking Editor, Mr. Stebbins, deserves our thanks for so valuable a contribution to our resources in this department of statistics.

8. *New American Cyclopedia*, vol. xiii, PARR—RED. Appleton & Co., New York, 1861.—This volume fully sustains the good character of its predecessors in the series.—*Photography*, by Dr. J. W. Draper, is a very interesting and instructive article. Of scientific articles, Parthenogenesis, Perpetual motion, Phosphorescence, Polarization, and Pneumatics, are furnished by Dr. Levi Reuben of New York; while Pearl, Perfume, Petroleum, Platinum, Pump and others are from Mr. J. E. Hodge. Prof. Parsons of Cambridge contributes a large number of learned articles on legal subjects. The article Persia, its Language and Literature, is from the learned pen of Prof. W. D. Whitney of Yale College. The article "Periodical Literature," by D. W. Fiske, Esq., of New York, is a comprehensive summary of that subject, and will be of the greatest importance to those seeking such information. Three volumes more we understand bring this series to a close.

9. *Personal.*—Prof. A. D. Bache, F.R.S., Superintendent of the Coast Survey of the U. S., was chosen Foreign Associate of the French Academy in the Section of Geography and Navigation in place of Mr. *W. Scoresby*, deceased.

OBITUARY.—Dr. ELI IVES died in New Haven, Oct. 8, 1861, at the age of eighty-two years. Dr. Ives was among the earliest of our botanists, and devoted himself particularly to the study of indigenous medicinal plants. He was the author of several botanical papers in the early volumes of this Journal. He was also an enthusiastic pomologist, and added several valued varieties to the cultivated Pears. He was one of the originators of the Medical Department of Yale College, and retained his connection with that Faculty for thirty-nine years. He was an eminently successful medical practitioner, and died at a good old age, universally beloved and honored by all who knew him.

Our list of publications received, and Proceedings of Scientific Societies, is unavoidably crowded over to an ensuing Number.

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